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# ROYAL AERONAUTICAL SOCIETY

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Edited for the Council by J. LAURENCE PRITCHARD, Fellow

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## NOTICES

### *Election of Members*

The following Members were elected at a meeting of the Council held on Tuesday, July 8th, 1924:—

*Associate Fellows.*—Mr. R. Vasconcellos de Aboim and Mr. K. W. Berger.

*Students.*—Mr. W. G. Kimber, Lieut. M. A. Maude, R.N., and Mr. G. S. Mills.

### *Lecture Programme,* 1924-1925

The following is the programme of lectures for next Session so far as at present arranged. It has been decided to hold all the lectures in the Society's Library, at 7, Albemarle Street, except in the case of those marked with an asterisk, which take place at the Royal Society of Arts, John Street, Adelphi. Advance copies of all lectures may be obtained from the Secretary, price 6d. each or 5s. for the whole series. Each lecture will begin at 5.30 p.m.:—

2nd Oct.—Lieutenant-Colonel H. T. Tizard, A.F.C., F.R.Ae.S., Chairman: Inaugural Lecture.

*16th Oct.—Dr. A. Rohrbach: "Large All-Metal Seaplanes."

30th Oct.—Major J. S. Buchanan, A.F.R.Ae.S.: "The R.Ae.C. Light Aeroplane Competitions."

13th Nov.—Professor L. Bairstow, C.B.E., F.R.S., F.R.Ae.S.: "Skin Friction."

27th Nov.—Dr. G. C. Simpson, C.B.E., F.R.S.: "Thunderstorms."

4th Dec.—Colonel F. Searle, C.B.E., D.S.O.: "The Maintenance of Commercial Aircraft."

18th Dec.—Mr. A. R. Watson Watt: "Recent Studies on Radiotelegraphic Atmospherics."

1925.

5th Feb.—Air Commodore C. R. Samson, C.M.G., D.S.O., A.F.C., A.F.R.Ae.S.: "The Operation of Flying Boats in the Mediterranean."

19th Feb.—Major R. V. Southwell, A.F.R.Ae.S.: (Title to be announced later).

5th March.—Lieutenant-Colonel C. B. Heald, C.B.E.: "Some Medical Aspects of Air Transport."

*20th March.—Dr. Eckener: "Modern Zeppelin Airships."

### *Associate Fellowship Examination*

Intending candidates are reminded that their entrance forms (together with the examination fee of £2 2s. 0d.) must be received by the Secretary not later than August 25th. Provided that sufficient entries are received the examination will be held on Monday, September 22nd (Part I.), and Tuesday, September 23rd (Part II.).

### *Wright Brothers' Medal*

Details are given on the next page of the conditions of award of a prize, in commemoration of the pioneer aeronautical achievements of the Wright Brothers, which has been inaugurated by the Dayton Section of the Society of Automotive Engineers.

W. LOCKWOOD MARSH, *Secretary.*

## WRIGHT BROTHERS' MEDAL

To commemorate the pioneer aeronautical achievements of the Wright Brothers the Dayton Section, Society of Automotive Engineers, will award a medal annually for the most meritorious contribution to aeronautical science reported to them during the year.

The contribution to, or discovery in aeronautical science and art may include advances in aerodynamics, development in construction of airplanes, power plants for same, accessories which increase their efficiency or reliability, such as fuel systems, control mechanisms, aerial navigation instruments, etc. Improvements for military application, ordnance, aerial photography, etc., are ineligible competition for this award. The prize will be awarded for contributions designed to stimulate increased non-military use of heavier-than-air craft.

Competition for this prize is open to any individual citizen or group of not more than two individual citizens of any country, excepting that S.A.E. National Officers and Dayton Section S.A.E. Officers, contest committee members, and award committee members are ineligible.

The award will be for intrinsic merit of achievement rather than merit of the paper describing it. Flight tests are desirable, and papers including complete reports of them, under proper conditions in the presence of unbiassed observers is advantageous to the competitor. Flight tests will not be absolutely insisted upon, as these might be financially impossible for individuals, though the proposed improvement would be clearly evident to a competent award committee.

Papers should be plainly labelled with author's name and address and forwarded to the Wright Brothers' Medal Committee, Dayton Section S.A.E., care of Engineers' Club of Dayton, Dayton, Ohio.

The Award Committee will be appointed annually by the Section Contest Committee:—

AWARD COMMITTEE 1925.

Prof. E. P. Warner, Mass. Institute of Technology.
H. M. Crane, President, Society of Automotive Engineers.

In addition an airplane designer and a test pilot, preferably secured from the air services of the army and navy, will be designated. Uncertainty of assignments in the air services makes announcement of names impossible.

The paper describing the achievement for which the medal is awarded shall be presented originally before the Dayton Section S.A.E., either by the prize winner, or if this proves impracticable, by some member of the local section appointed by its officers.

The award for 1924 shall be based on papers received up to December 31st, 1924, and shall be announced as soon as all papers and data therein can be checked. The Dayton Section S.A.E. reserves the right to withhold any award if the Award Committee finds no paper submitted of proper standard.

The Dayton Section S.A.E. reserves the privilege in future years of restricting the competition to a narrower field of endeavour than is announced for 1924.

However, any such restrictions shall be announced by January 1st of the year during which they are operative.

WRIGHT BROTHERS' MEDAL COMMITTEE, DAYTON SECTION, S.A.E.,
*Engineers' Club, Dayton, Ohio, U.S.A.*

# PROCEEDINGS

## TENTH MEETING, 59TH SESSION

A meeting of the Royal Aeronautical Society was held at the Royal Society of Arts, John Street, Adelphi, London, on Thursday, March 6th, 1924, Colonel A. Ogilvie (Chairman of the Society) presiding. A paper was read by Major W. S. Tucker, D.Sc., A.M.I.E.E., on "Sound Reception."

The CHAIRMAN, in introducing Major Tucker, said that his name would be known to many of those present, in view of his association with the hot-wire microphone.

Major TUCKER then read his paper.

## SOUND RECEPTION

The growing importance of the study of Sound has been shown in many ways during the last few years and most particularly in its applications to military intelligence. Since the classical experiments of Tyndall[1] in 1872 and following years, there is no very definite record of open-air experiments until L. V. King[2] in 1919 performed some experiments for the Canadian Government on fog horns.

A new era of investigation has now been opened up by the realisation of the importance of observation by sound during the late war. The distant gun was located very effectively on most of the Army fronts, and towards the latter end of the war the aeroplane, invisible at night, was being located by sound also.

The urgency of the problem to be solved resulted in the development of new and effective apparatus which is now available for peace applications as they arise. New microphones and new electrical accessories, particularly the new thermionic valve, have given acoustical research a new interest and have led to the hope of our being able to produce standards, thus bringing sound into line with the other branches of physics.

The Royal Aeronautical Society would naturally be most interested in sound as applied to aircraft, and a very big field is opened up when we consider that the pilot first discovers engine trouble, as a rule, by sound; that the passenger suffers more discomfort from sound than from any other cause, and that when air raids are imminent it is our faculty of listening which is first called into play. How much more important this faculty will become in the future it is impossible to say, but we know that the most effective raids will be carried out after dark or under the protection of cloud, and under these circumstances our early warnings and our locations can only be obtained by acoustical methods. Much attention must, therefore, be devoted to anti-aircraft acoustics as the only means of locating aeroplanes.

A small research and experimental section supported equally by the three fighting services is devoting all its energies to the solution of such problems as Sound Reception for the purpose of anti-aircraft defence, and although much of its work is of highly confidential character, there are certain general principles which are already known to the services, or have been recently discovered by research, which may be communicated, and which have the virtue of being new to the average technical audience.

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[1] Tyndall. Sound Lecture VII.

[2] L. V. King. On the Projection of Sound on the Free Atmospheric and the Acoustic Efficiency of Fog Signal Machinery, etc. Phil. Trans., Series A 218, pp. 211-293, 1919.

In order to deal as fully as possible with my subject of Sound Reception, I might divide this paper into three sections:—

(1) Aural Reception.
(2) Electrical Reception.
(3) Transmission through the medium, *i.e.*, the air.

## I *Aural Reception*

The ear as an instrument for listening has been the subject of study from earliest times and yet is very imperfectly understood. The internal ear, where the mechanism of listening is confined, is almost inaccessible. Internal ear deafness is nearly always incurable. It is not surprising, therefore, that theories of listening are very speculative. A casual inspection of a diagram of the ear shows how complicated it is (Fig. 1). Some hold the view that such complications as

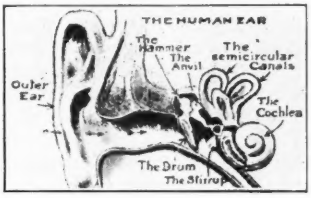


FIG. 1.

the ossicles in the middle ear are survivals of a more primitive form in which their functions were more clearly defined. We know that the malleus and the incus can be removed without creating absolute deafness, although the absence of these is associated with partial deafness. A discussion on the apparatus for analysis of tone situated in the inner ear has raged for sixty years, and is still being carried on. The famous theory of Helmholtz, who combined the two subjects of physics and physiology in his famous book on "Sensations of Tone," has been discredited by recent authorities, but the theories which have been put forward to replace those of Helmholtz are themselves still subject to various criticisms.[3] In the matter of discrimination of the different sounds, the tendency is to throw more and more responsibility on the brain, and less on what might be called the physical apparatus. Separation of sound impressions and their analysis would be attributed to psychological rather than physical processes.

Nevertheless, the mechanism of the ear is very interesting. The membrane of the tympanum which receives the sound and is set in vibration by it is a veritable diaphragm responding to sounds imposed on it just as does the telephone transmitter diaphragm. By a system of muscles this diaphragm can be tightened at will, thereby reducing its sensitivity.[4] Unlike the telephone diaphragm, however, it is very poorly resonant, the reason being that it is so heavily loaded by its attachment to the first of the chain of small bones. The middle ear is the region across which sound is conducted to the inner ear. Here again there is a long-standing controversy as to the mode of conduction.[5] Vibrations have to be transmitted to the cochlea of the inner ear, which is separated from the middle ear cavity by two membranes. The larger one, fenestra ovalis, is connected to

[3] An Inquiry into the Analytical Mechanism of the Internal Ear. Wrightson.
[4] Zünd-Burguet. Conduction sonore et Audition. Tome I.
[5] Marage. Rôle de la chaine des osselets dans l'audition. Bulletin acad. med. Paris, 1900.
Müller. Berichte d. Sachs Gesellsch. der Wissensch. 1861.
Secchi. La fenestra rotunda e la sola via dei suon dall aria al labyrinthe. Torino.

the ear drum by the chain of small bones, and it was always considered that this chain was the path by which the sound energy exciting the ear drum was transmitted. This view, however, has been strongly contested by a large number of authorities on the ground of the mechanical inefficiency of such an arrangement since the medium of transmission, the somewhat soft nature of the bones forming the ossicles, the still softer connecting tissue and the heavy drag exerted on these small bones by the muscles which hold them in place, are ill-adapted for the transmission of rapid vibrations.[6] The alternative suggestion is that the sound is transmitted through the air of the middle ear almost entirely and received by the fenestra rotunda. The issue between these two opposing theories is not yet decided. Undoubtedly, the fact that deafness does not entirely follow from the breaking of the chain of ossicles shows that some sound may be air-transmitted, yet it has been proved that hearing has definitely benefited by inserting a thread connection between the tympanic membrane and either of these two inner membranes. The function of the ossicles, however, has been asserted to be a means of converting one type of vibration into another. The tympanic membrane vibrating in air receives air displacements with small pressure change. The system of levers of the ossicles converts these relatively large displacements with small pressure change into small displacements at the fenestra ovalis and large pressure changes.[7] The latter type of vibration is more suitable for transmission into the liquid which fills the cochlea behind that membrane. The disturbance after passing to the end of the spiral cavity of the cochlea passes back to the fenestra rotunda, which, being elastic, bends outwards as the fenestra ovalis bends inward and *vice versa*.

Before considering the function of the inner ear, one other interesting phenomenon may be referred to. The tympanic diaphragm must be subject to a certain distension for effective listening, and this is produced by a small increase in pressure of the air within the ear drum brought about by the act of swallowing through the opening and closing of the Eustachian tube which connects the ear drum to the mouth cavity. Secchi showed that sound incident on the ear increased this internal pressure to an extent depending on the intensity of the sound, this increase being due to reflex action.[8]

If the mechanism of the middle ear is complicated, that of the inner ear is much more so. The seat of the analysing mechanism by which the pitch and quality of a sound are recognised appears to be the basilar membrane with its thousands of small structures called the rods of Corti. Each little structure was at first regarded by Helmholtz as a tuned receiver, each capable of responding to its own short range of musical pitch. It would be inopportune here to enumerate reasons for the discarding of Helmholtz's theories by modern authorities, or to describe the alternative theories, all of which have their critics.[9] It is sufficient to say that each little mechanism probably has its function as a piece of physical apparatus and each contributes to the marvellous efficiency of the ear as a receiver of sound. In two respects the ear with its nervous system and its brain interpretation has the advantage over any artificial receiver. It has wonderful power of accommodation, and it has power of discrimination between two different classes of sound, irrespective of pitch.

With regard to "accommodation" or power of the ear to function with sounds of greatly varying intensity, it has been suggested that this is effected by the tightening of the ear drum by the controlling muscles attached to the ossicles.[10] Whether the process of desensitising the ear is mechanical, psycho-

[6] Zünd-Burguet, *ibid.*
[7] Zünd-Burguet, *ibid.*, p. 200.
[8] *Ibid.*, p. 182.
[9] Fletcher. Journal of the Franklin Institute, Vol. 196, Sept., 1923, p. 289.
[10] Zünd-Burguet, *ibid.*, p. 201.

logical, or a combination of the two, it is certain that sounds varying from that of extreme faintness to one 100,000,000 times as great can be appreciated, and that the ear can carry on its function of listening unimpaired even after receiving so considerable a strain as that indicated by the latter quantity. Sudden intense sounds, such as explosions, produce very painful sensations and cause temporary deafness, but if time is given for accommodation no such results occur.

During the war the sudden explosion of a single field gun near at hand was more painful than the continuous effect of many batteries of field guns equally near firing a barrage. The employment of ear defenders such as the Armstrong-Mallock ear defender, consisting of an extra rubber diaphragm mounted outside the tympanic diaphragm, serves as an efficient protection against sudden explosions, but deafens the listener to ordinary sounds (Fig. 2). The anti-aircraft

FIG. 2.

*Section of Mallock-Armstrong Ear Defender.*

listener, employing listening trumpets or mirrors, is very much exposed to injury from the sound of neighbouring explosions, but no device is yet available for enabling him to listen to very faint sounds, as of a distant aeroplane, while protecting him from these explosive effects.

The excellence of the ear as a piece of listening apparatus can be summed up by quoting a few figures. For whistling notes of a pitch of 1,000 vibrations per second, the ear can appreciate sounds of pressure amplitude varying from 1 to $10^8$ (a hundred million units) and can appreciate 400 gradations of loudness.

It can distinguish between sounds having a ten per cent. variation in intensity except for the low notes of the scale when the variation may increase to 30 per cent. As regards pitch between 400 and 4,000 per second, a difference of .3 per cent. is capable of recognition. For lower notes, however, it may increase to one per cent.[11]

For notes varying from 50 vibrations per second to 3,000 vibrations per second, it is possible to recognise 640 different pitches, and this corresponds to about six octaves or 72 notes of the piano.

On account of the very large range of values (.0001 to 10,000 dynes per sq. cm.), and on account of the extended range of frequencies 20 to 20,000 vibrations per second, it is better to use logarithmic scales to indicate the quantities; thus for pressure amplitudes we may employ powers of ten as units, and for pitch powers of two.

The threshold values of audibility, *i.e.*, those values of intensity at which the sound is just audible, can be shown on a curve for different frequencies, and it is seen that the ear is most sensitive to a frequency of about 2,000 to 3,000 vibrations per second (Fig. 3).

[11] Knudsen. Phys. Review, Jan., 1923, p. 84.

An upper limit to intensity can also be given at that point at which one "feels" the sound, the sensation being described as a "tickling" sensation. Beyond this point further increase of sound gives pain to the listener. This threshold of feeling is said to be quite definite and is shown by the upper curve in the diagram. The sensation of feeling a sound is experienced when listening to the lower pedal notes of the organ.

Each sensation of pure sound is now represented by some point in the area enclosed between the upper and lower curves, and it has been estimated that the ear is capable of discriminating as regards pitch and intensity 300,000 pure musical tones.[12] This is a remarkable performance and it is not a matter of surprise that we are very exacting in our requirements of a musical instrument. No microphone can compete with such range and efficiency.

The ear properly trained can exercise the power of discriminating between different sounds; thus the trained anti-aircraft listener can distinguish between the sound of a rotary engine as compared with the stationary engine of an aeroplane, although to a casual observer there may be little noticeable difference between them.

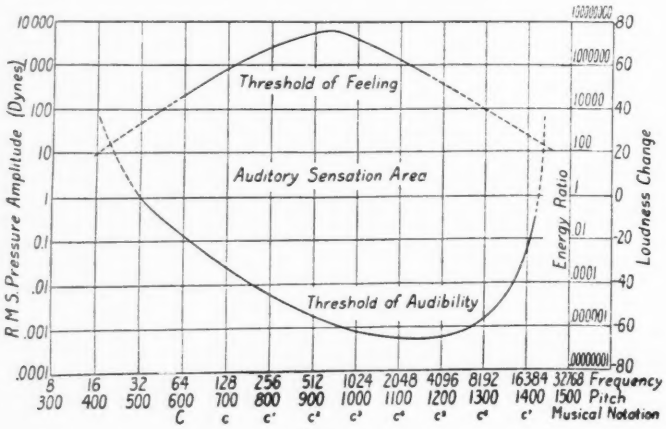


FIG. 3.

One other characteristic of the ear is its power of hearing a given sound in the presence of others. It was pointed out by Mayer[13] that high tones can be completely masked by louder lower tones, while intense higher tones are incapable of obliterating the lower ones. This general statement needs some qualification as shown by Fletcher,[14] in so far as the high ones, far removed in pitch from the low ones which mask them, require the latter to be of increasing loudness as the interval between them increases. This has some bearing on the sound of an aeroplane as we perceive it. The very low notes of the exhaust and propeller are always audible in spite of the very large amount of energy in sound put out by the higher pitched notes; in fact, as both sounds diminish in intensity with a receding aeroplane, it is the lower note always which attracts attention. Another reason for the predominance of the lower note for long ranges of listening will be given later.

One of the most characteristic aeroplane sounds is the "beat" produced when the two engines of a double-engine machine are not quite performing the

[12] Fletcher. Journal of the Franklin Institute, Vol. 196, Sept., 1923, p. 289.
[13] Phil. Mag., Vol. II., p. 500, 1876.
[14] Fletcher, *ibid.*

same number of revolutions. The "beat" has a periodicity which is the difference of the periodicities of the explosions in the cylinders, or the difference in the number of revolutions of the two-bladed propellers driven by these engines. It is a well-known fact in psychology that a very faint sound which is continuously produced can be imperceptible until this sound is interrupted, and repeated interruptions such as these beats represent will make the low tone of an aeroplane audible in spite of the lower disturbing sounds, which would tend to render it imperceptible. The beats referred to were characteristic of the "Gotha" so frequently heard in the later air raids on London. The presence of beats is a very great help to anti-aircraft listeners.

If we now deal with the location of aeroplanes, the operation to the listener, unaided by special apparatus, is performed by employing the two ears and exercising what is generally called the binaural faculty. The transmission of sound through the air by waves of alternate compression and rarefaction with a speed just suitable for this operation enables the two ears to set themselves so that at the same moment they receive the same kind of disturbance. This is only realised if the listener faces the sound. As the listener turns his head there is a retardation of the arrival of that disturbance which is best expressed by saying that there is a phase difference between the waves arriving at the two ears. The ear nearer the source of sound appears to hear it more strongly, although, from our knowledge of power to discriminate intensities, there is probably an imperceptible difference in that intensity, especially for the lower notes of longer wave length. A certain screening effect of the head may reduce the intensity of the sound in the ear which is turned away from the source, and no doubt the listener takes full advantage of it in getting direction, but it is generally accepted that direction is determined chiefly by the phase difference effect.[15] This sensation of increased loudness in the near ear is not easy to recognise, but it may be rendered quite evident by the use of listening trumpets. Thus, suppose that the listening orifices are not the ears but the mouths of conical trumpets with parallel axes, and that the base length between the ears (say, five inches) is replaced by span of five feet between the trumpet orifices; then, when stethoscopes connect the trumpets to their respective ears, we have a magnifying listening apparatus with all the advantage of a longer base. The sound is louder, and a given turn of the trumpets produces a larger phase difference between the sounds than would be procured by the ears acting on opposite sides of the head. The trumpets may be rigidly mounted on a frame so that the pair may be rotated about an intermediate axis perpendicular to the trumpet axes. This corresponds to turning the head, and when the ears are thus aided by the trumpets the "sensation of passage" from ear to ear is obtained, the sound appearing to pass round the back of the head. The pair of trumpets constituting the locator should be oscillated rapidly until the sound is bracketed, the oscillations gradually being reduced. The bearing of the trumpets will give the direction of the sound.

From any given source of sound such as an aeroplane, the phase difference effect is greatest when the elevation of the aeroplane is very small, *i.e.*, near the horizon. As the aeroplane approaches and the elevation increases, the axes of the listening trumpets should be tilted up until for the overhead position they are parallel to the axis of rotation. It will then be seen that for a given angular swing of the trumpets the variation of position of the source relative to the trumpets gradually reduces as the elevation increases, and for the overhead positions the trumpets keep the same distance even for a complete revolution.

When only two such trumpets are used we have an arrangement analogous to the human binaural device, and it will be seen that the location of an aeroplane as regards bearing for the overhead position is extremely difficult; in effect, it will be noticed that an ordinary listener can point with good accuracy to an aero-

[15] Rayleigh. Phil. Mag. 13, 1907, pp. 214-216. Stewart. Phys. Review, June, 1917, p. 502.

plane near the horizon, but cannot estimate exactly when an aeroplane is above him. This zenith difficulty probably accounts for the amount of terror inspired in the average listener by a bombing aeroplane which for quite a considerable interval of time appears to him to be exactly overhead. In order to define completely the position in space as regards angle, two sets of listening trumpets and two listeners must be employed—the first one obtaining bearing by trumpet pairs rotating on a vertical axis, the other obtaining elevation by another pair of trumpets rotating in a vertical plane whose orientation is determined by the first listener. The diagram Fig. 4 shows such a mounting. Even now the zenith

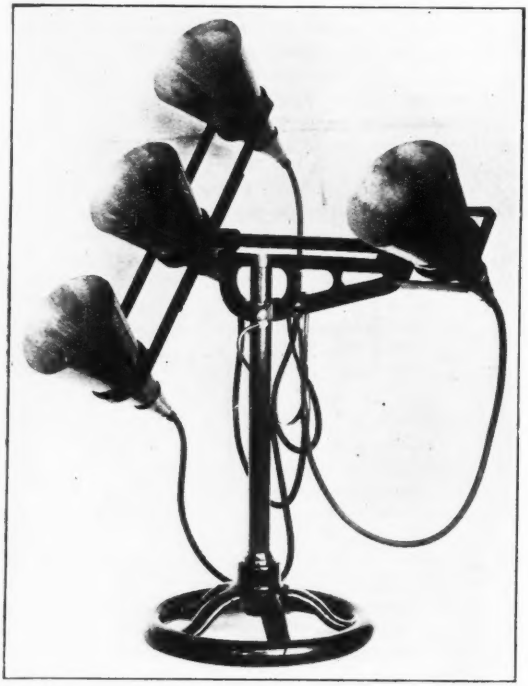

FIG. 4.

difficulty is not overcome, for although elevation can be accurately obtained for the overhead position, the bearing may be quite wrong. When one remembers that the apparent movement of the aeroplane overhead is greatest, it will be seen that when an aeroplane is in its most dangerous position its location is most difficult to fix. This altazimuth mounting of trumpets lends itself to scale readings more simple to transfer to searchlight and to gun, and therefore is the one most employed.

An alternative mounting employed by the French and incorporated in an instrument designed by Perrin is known as the Bennett mounting. Such mounting is indicated in Fig. 5, which is the reproduction of a model. There are three axes—one vertical, one horizontal, and a third, by virtue of the other two, capable of occupying any position in space. If this latter axis is discarded we have, of course, the type described above, provided the plane containing all the trumpet mouths is parallel to the horizontal axis. It is, however, the practice to select

the direction in which we think it most convenient the horizontal axis should point and then clamp the vertical axis. We then have two axes about which rotation can take place—one parallel to the plane of the trumpet mouths, and the other perpendicular to this axis. It will be seen now that uncertainty in location is transferred to two regions of the horizon which are prolongations of the horizontal axis, and that greatest accuracy of location is given by the zenith position.

One position of the trumpet axes makes for great simplicity, viz., that in which the horizontal axis is parallel to the course of an aeroplane (supposing this course to be straight). Only one listener gets changes of reading as the flight proceeds. This case, however, is dependent on the straight course above referred to.

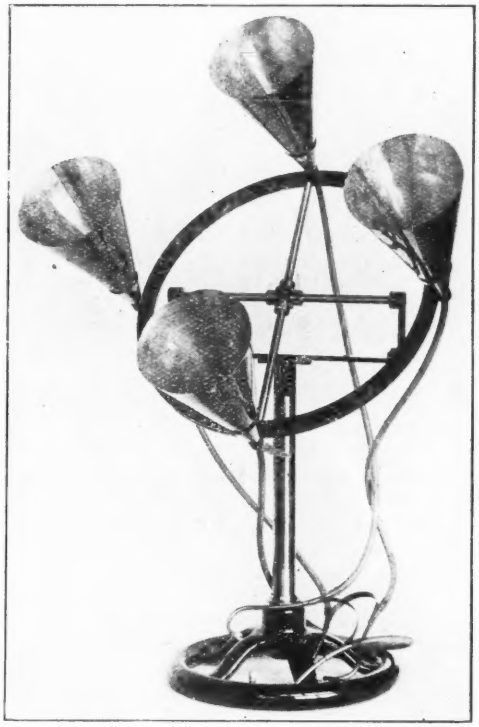

FIG. 5.

A great difficulty in this type of mounting is the suitable setting of scales for observing readings, and a further difficulty is experienced in the mounting of searchlights so that sound locator readings can be transferred.

It will be quite obvious that the use of a sound locator of either type is a matter of skill and training. In the matter of discrimination between a number of aeroplanes, the phase difference method makes interference between the incoming sounds a serious factor, and it is possible for the two listeners to be working on different targets simultaneously. Each listener has control of his own trumpet movements, but it is only by practice that each can function without suffering some constraint from the movements of the other.

The models exhibited in Figs. 4 and 5 must only be considered as types. They bear little resemblance to service instruments. Furthermore, the means for correcting lag of sound, which affects readings materially, has been purposely ignored as being beyond the scope of this paper.

Before passing from this subject of trumpet reception, a few remarks must be made about the failings of trumpets. No proper conception of the magnifying power of a trumpet for listening can be obtained apart from the frequency of the sound which it receives. Trumpets magnify high-pitched sounds when pointing towards them far more than low-pitched sounds. They magnify variously for various pitches and for various directions. This shows up best in a diagram Fig. 6, but not only do the trumpets show these gradual variations as pitch alters

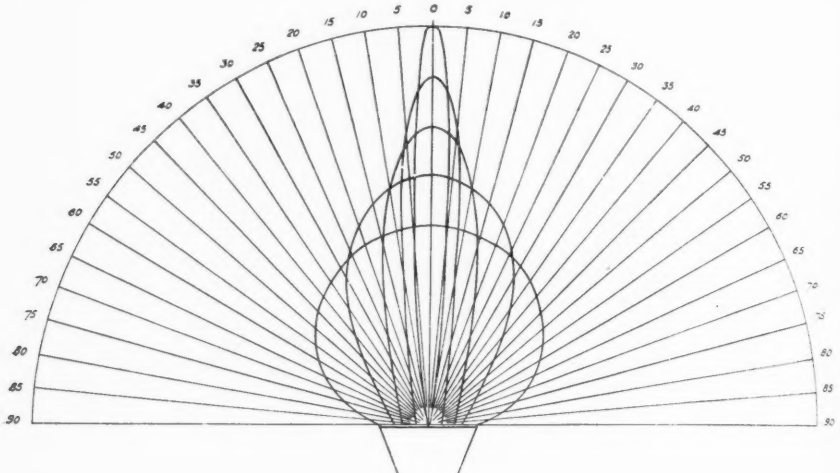


FIG. 6.

—they also have certain favourite notes which they magnify out of all proportion. These are the resonance notes of the trumpet. The diagram does not show a resonance condition, but it will be seen that if no such resonance notes are introduced into our source of sound, the quality of the sound alters with rotation of the trumpet—the high-pitch sounds are quickly lost, and the low notes become predominant, until ultimately, when the trumpet is well inclined to the sound, only the lowest notes are heard. It will be readily understood from the above statement that the sound of the aeroplane as heard through the trumpet is altered almost out of recognition, and it becomes difficult to identify aeroplanes when listening through trumpets. The effect is even more pronounced if the aeroplane provides a note to which the trumpet is resonant, for this one strongly predominates. Moreover, for such notes our binaural faculty is put out of action, because resonance upsets the phase relationships upon which sense of direction is based. It will be evident, therefore, that the design of a trumpet sound locator is a matter of some care and that a good listener needs careful training.

The size of a trumpet and the cone angle is also a matter requiring careful consideration. A great deal of work has been done by Stewart in America and Waetzmann in Germany, and certain results may be quoted.

Stewart[16] used horns 18 feet in length, with diameter of opening 4.5 feet, and found with them that he could get a range three times that of the unaided ear. The difference between this and the amplification of 100 obtained by local experiments, led him to the conclusion that much sound was lost by diffusion in the atmosphere.

Waetzmann,[17] using a trumpet two metres long, increased listening range by 50 per cent. for aeroplane sounds.

Service trumpet sound locators, as used in the war, were the outcome of experiments conducted by the Anti-Aircraft Experimental Section of the Munitions Inventions Department, under the Directorship of Major A. V. Hill.

Before passing from the consideration of aural methods, some reference must be made to the use of sound mirrors. These to be effective must be very large, so large, in fact, that there is very little advantage in using them in altazimuth mounting as with the trumpets.

The French employed a limited number of mirrors designed by Baillaud and made of plaster on an iron framework. These were three metres in diameter and of one metre focal length. They were parabolic in shape and the sound was collected by trumpets and stethoscopes mounted round the focus and so arranged that one listener obtained bearing and the other altitude—the mirror being rotated until each listener gets a balance of intensity in the two ears, indicating the bracketing of the focus. It was generally agreed that the arrangement was too clumsy for quickly moving aeroplanes in the vicinity of the mirror.

If, however, the mirrors are fixed, the dimensions and the magnifying power may be increased. The location must now be obtained by the movement of collecting trumpets.

The mirror must be spherical and for an aeroplane emitting sound in front of the mirror there will be a sound image produced at a focus. This focus will be in line with the source of sound and the centre of curvature of the mirror, and be about halfway between that centre of curvature and the mirror surface. It will be seen, therefore, that if the mirror surface is spherical the foci will all lie in a concentric spherical surface of half the radius of curvature. If the trumpets for collecting sound and locating the focus are mounted on a swinging arm pivoted at the centre of curvature they can be rotated and give by their bearing and elevation the position of the sound image.

The sound image of the aeroplane is not very definite on the low notes, but becomes sharper and sharper as we concentrate on the higher pitched sounds. Whispers can be located with great accuracy, and the sharpness of the image can be related to the wave-length of the sound (or its frequency) in very much the same way as that shown in Fig. 6.

With the Baillaud mirror the lowest notes get a magnification of about one and a half, running up to five for a high-pitched source.

Waetzmann[18] made a parabolic mirror of 10ft. 6in. diameter and about 31in. deep (Fig. 7) and found the magnification of five for whispers, but less for lower notes.

A very satisfactory parabolic mirror was cut out in the chalk at Binbury Manor, near Maidstone, by Mather and Irwin. The diameter was 20ft. and the focal distance 5ft. and good magnification was obtained.

Concrete makes an excellent material for mirror construction. Fig. 8 shows an experimental mirror used by the Acoustical Section for both reception and transmission of sound. The diameter is 20ft., the depth about 4ft. 6in., and the radius of curvature 15ft. An operator with his head 7ft. 6in. from the mirror

[16] Stewart. Phys. Review, Aug., 1919, p. 167.
[17] Waetzmann. Zeitschrift fur Technische Physike, No. 7, 1921.
[18] Waetzmann. Zeitschrift fur Technische Physike, No. 7, 1921.

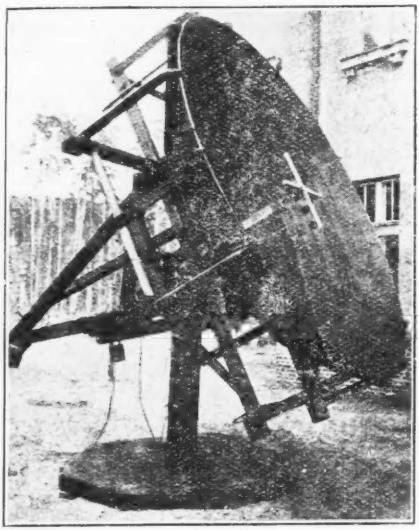

FIG. 7.

FIG. 8.

can exchange conversation with another man 400 yards away without effort. Care must be taken, however, to get the right position both for listening and speaking. Magnifications are then about ten times for both reception and transmission, in fact the sound beam transmitted by the speaker in the mirror has an angle of dispersion of about 12° in which all the audible sound is confined.

A mirror so constructed can, of course, only deal with sounds on one side of it and within a certain range of bearing on either side of its axis, yet its powerful magnification renders it a very useful directive sound receiver.

We will now pass to the second heading, "Electrical Reception." A good case for its development is already indicated in the above analysis of aural methods. Trained listeners are difficult to obtain and experience has shown that acute listening with full exercise of the binaural faculty cannot be carried out over periods exceeding half an hour at a time. Heavy demands on personnel must be made to maintain an efficient listening service for anti-aircraft work. The desirability of using apparatus is therefore obvious, especially if it can be adapted to give automatic indications of the aeroplane's position and direction of travel.

The results obtained with commercial microphones have been extremely meagre. It would appear that no microphone of a periodic character can compare with the human ear, even when associated with the most efficient amplifier such

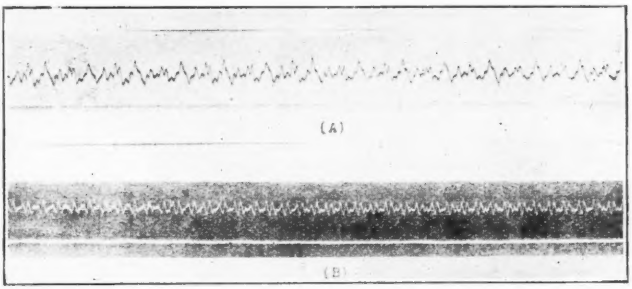


FIG. 9.

as that employed in radio telegraphy or telephony. Probably no more efficient carbon microphone has yet been employed than that described by Waetzmann, and Fig. 9a shows a photographic record of a seaplane Albatross with Mercedes engine at a range of 1,000 metres, using a large trumpet collector, and Fig. 9b that of an aeroplane using microphone without collector, the range for which is not quoted.

Both records are no doubt complicated by the resonant character of the receivers, especially the former, in which trumpet distortion must be produced. The most interesting point in each record is the change in character of the sound as the flight proceeds. Fig. 9b is a record of three seconds duration and shows large variation in the size and disposition of the peaks. This is due partly to Doppler effect and partly to change in the nature of the sound as the aspect of the aeroplane alters.

An attempt was made by Lübcke[19] to devise microphone apparatus for detecting an aeroplane from the periscope of a submarine, using a similar microphone but with a certain degree of tuning to make it more sensitive to aeroplane notes. The experiment was not successful owing to the difficulty of amplifying up with valve amplifiers. The latter are essential, but the microphone noise is

[19] Lübcke. Zeitsch. fur Technische Physike (2), 1921.

too prominent to give any advantage. Some success, however, was obtained by using the telephone receiver, which is an electro-magnetic microphone. Distortion of the sound is of course produced, but ranges nearly those of the unaided

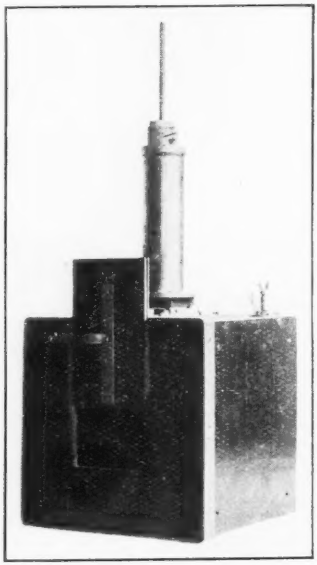

FIG. 10.

ear were obtainable. For long distance listening it is obvious that resonant microphones must be employed, but they cannot be employed effectively unless the source of sound has a predominant note or range of notes to which the

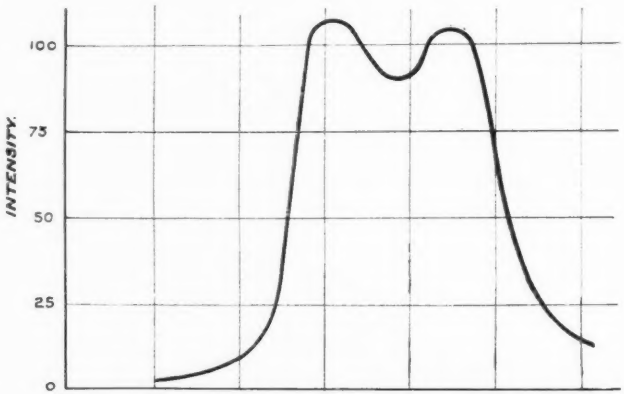


FIG. 11.

*Double Resonance Curve of a Tuned Double Resonator.*

receivers can be tuned. Fortunately the hum of an aeroplane is very well defined and its pitch can be identified with some degree of success.

The microphones now being produced for anti-aircraft work cannot be described here, but the principle of resonance has been employed in the design of microphones for observation on Trinity House foghorns. A full description of the microphone is given in "Journal of the Royal Society of Arts"[20] and a

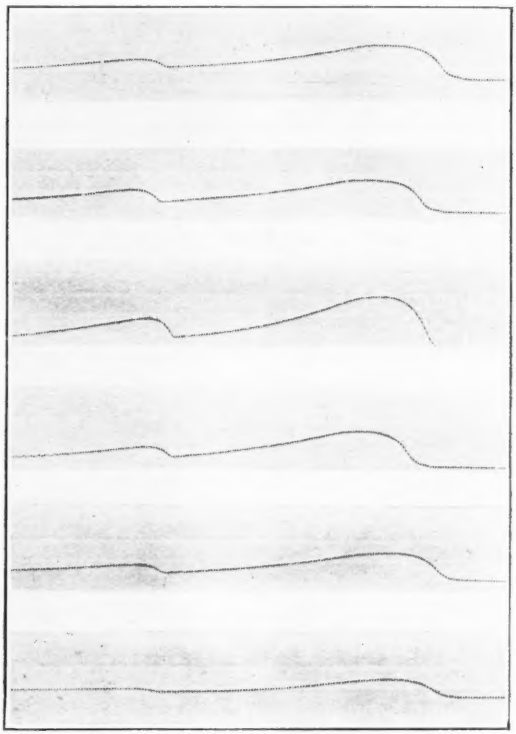

FIG. 12.

*Records of Sound Received from the Siren of the N. Goodwin Light Vessel. Range 7 Miles. Date July 7th, 1921. Time 4.30 (Summer Time). Larger Peak, Low Note. Smaller Peak, High Note. Apparatus Tuned to Low Note. Successive Blasts at One Minute interval increasing in strength to Three and then diminishing.*

picture of it is given in Fig. 10. The sound falls on the open window of a box, in the walls of which the neck of a cylindrical bottle is inserted. When both box and bottle are tuned, the combination is set in resonant vibration by the sound and the curve of response to notes of different pitches within the range of the instrument is shown in Fig. 11. Any sounds lying outside this range are incapable of producing an effect. In order to detect and measure the vibration produced in the combination, a hot wire grid is stretched across the neck of the bottle and

[20] Journal of the Royal Society of Arts, Jan. 5th, 1923.

variations in its electrical resistance resulting from cooling due to the vibrating air are observed and measured by appropriate electrical means. By one method of using the instrument the amount of sound received is indicated by uni-directional electric current so that the record obtained cuts out the "crests" and "troughs" of the sound wave, giving a steady deflection for sound of a given strength. This property of the microphone enables one to use simple apparatus. If the sound varies slowly, as in the case of "beats," these variations can be observed.

FIG. 13.

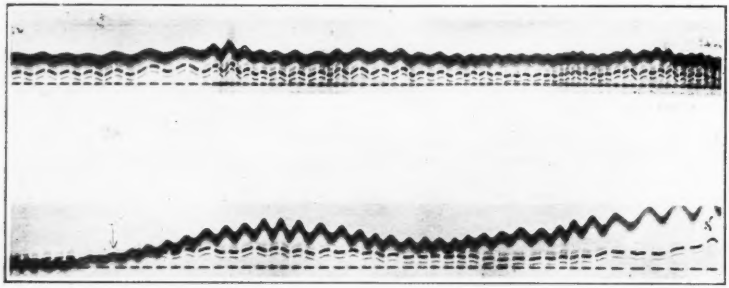

FIG. 14.

By way of illustration, Fig. 12 shows a series of records of the siren of the North Goodwin Light Vessel. The record shows two blasts and the smaller one is of high pitch, but as the blast is produced the sound passes through the note of the lower-pitched blast. The microphone is tuned to the latter, but no amplifier is used. The range is seven miles. The records show a succession of blasts at minute intervals and their apparent variation is due to atmospheric absorption, to which reference will be made later.

Fig. 13 shows a record of a Vickers-Vimy double-engine machine with Rolls-Royce engines. The upper continuous line shows the exhaust sound and the lower line shows that of the propeller. Here the definite advantage of tuning can be appreciated. The "beats" between both engines and propeller sounds are clearly shown. The distance between the breaks corresponds to seconds and the range is from one to two miles. Fig. 14 is part of a record of a Vickers-Vimy flying transverse to the direction of listening at a range of from four to five miles. Here again the two sounds are separated.

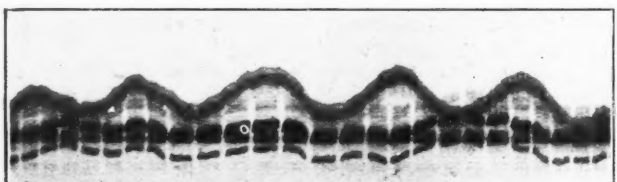

FIG. 15.

Fig. 15 is a record of two Vickers-Vimy machines. One machine exhibited beats at an interval of five seconds, showing a difference of engine revolutions

between the port and starboard engines of 20 revolutions per minute. The other machine was running its engines in tune, but the average speed was 60 revolutions per minute greater. In consequence of this, two ripples are shown on the record, one of five seconds interval corresponding to the first machine, the other between the first and second machines of one second interval. The indications of the sound are in agreement with the logs of the two pilots. The range of both machines was about three miles at the moment the record was taken, and they were then nearly a mile apart.

### *Transmission of Sound Through the Air*

With the best apparatus for detecting and locating aeroplanes very disappointing results are sometimes obtained. Everyone is familiar with the great variations experienced from day to day when listening to familiar sounds at long ranges, and in order to complete the survey of this subject of sound reception, consideration should be given as to what are the weather conditions making for good or bad transmissions of sound.

A careful study of the subject shows that we may get sounds for quite long ranges with good sense of direction on some days—on others equally good ranges without that sense. On some days when listening to aeroplanes, and especially at night-fall, the quality of the aeroplane sound is more clearly defined —on others, only the lowest pitched notes appear to come through.

So close an analogy exists between the phenomenon of sound transmission and light transmission that the same language may be used in the two cases. Refraction and diffraction of sound, however, occur on a much bigger scale. Sound shadows are less definite, sound images resulting from mirrors, etc., are in general less well defined. This is partly due to the smallness of the apparatus employed as compared with the wave length, and partly to the comparatively large variations in velocity of sound when passing through the air as a medium.

If we take the sound mirror of large dimensions as an example, it has been found to produce fairly good sound images. Different types of aeroplanes can be recognised and aeroplane sounds, inaudible to the unaided ear, can be located with good accuracy. There is, however, considerable variation in the ease with which such locations can be obtained. This variation in location is due to the condition of the atmosphere, and the same difficulty in location would undoubtedly be experienced by the ordinary listener trying to use his own binaural faculty. The atmosphere contains "acoustic clouds," masses of air impregnated with water vapour to varying extents, or possibly merely differing as regards their temperature, and in addition it may contain vortices of varying magnitudes both as regards rotating velocities and actual dimensions. All these variations may be regarded as a kind of "acoustic dust," the source appears "blurred" acoustically and cannot in the nature of things give an image other than blurred. Conceivably the sound may be as highly "scattered" as sunlight is scattered on a cloudy day, and it might be as difficult to locate an aeroplane under such conditions as it is to locate the sun when the light is entirely diffused. This difficulty is frequently experienced on a clear summer day, for the acoustic clouds may be invisible. There is an "acoustic fog" frequently associated with good visibility. On the other hand, a uniform fog, which makes aeroplane flights impossible, is frequently associated with acoustically clear weather. Under this condition there is complete absence of vortices and a very uniform temperature condition. Sound direction-finding apparatus then becomes very efficient, and it is remarkable that use is not made of this fact for navigation in fog. Many collisions at sea might have been avoided with recognised signals and simple trumpet locators.

In order to estimate the probable amount of absorption, special apparatus is being devised for examining the structure of the atmosphere. Very little advan-

tage can be derived from a knowledge of the average temperatures and humidities quoted in meteorological reports. Reverting to Fig. 12, the variation in intensity of the sounds of the siren blasts is seen to be considerable, although they are only separated by intervals of a minute. These variations are due to acoustic clouds.

Fig. 16 is the record of a Vickers-Vimy aeroplane in which the variations

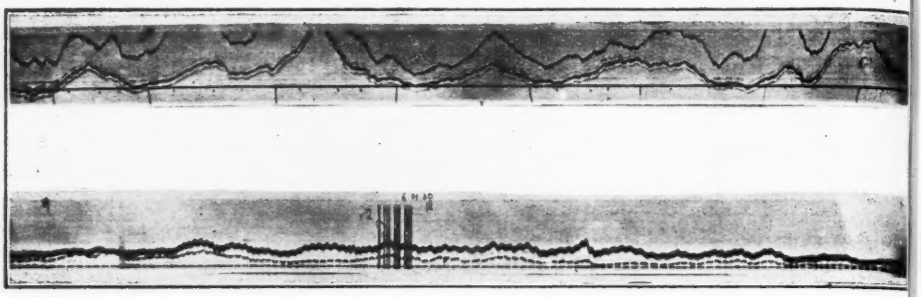

FIG. 16.

in average intensity are also due to acoustic clouds, and it will be noted that both exhaust and propeller sounds are similarly affected. These clouds affect the high-pitched sounds more than those of lower pitch, hence the persistence at long range of the low note of an aeroplane and the extinction of its non-musical sounds. Sometimes banks of acoustic cloud occur which cause complete extinction of the sound, and such a case is shown in Fig. 17, where the sound is extinguished if

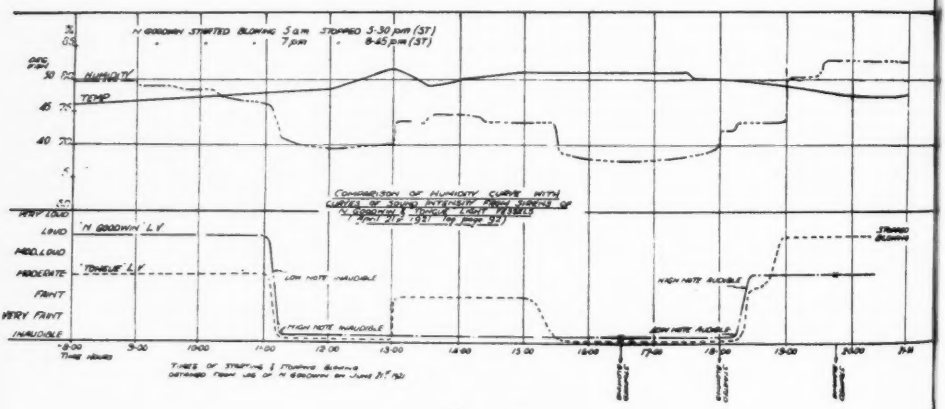


FIG. 17.

the surface of separation of cloud and clear air lies between the source and the observer. If both source and observer are enveloped either in cloud or in clear air, the sound is audible. In the record shown the limits of the cloud are indicated by hygrometer readings.

Range of listening is also affected by refraction, either through wind or temperature variations. This property was investigated by Rayleigh,[21] and more

[21] Theory of Sound (Vol. II.), p. 129.

recently Milne has given a very complete treatment of the subject.[22] The temperature distribution in the atmosphere has the effect of elevating the "acoustical horizon," except under very abnormal conditions, and the variation in strength of wind on ascending, may either elevate or depress this horizon according as the resulting velocity of sound from the source to the observer, decreases or increases as the height increases. This phenomenon of elevated horizon shows itself in the sudden "coming in" of the sound from an aeroplane when it is well above the visual horizon. On the other hand, the acoustical horizon may lie below the visual one, thereby giving indefinitely high ranges for listening, if suitable magnifying apparatus is employed.

Absorption and refraction are both very serious handicaps to anti-aircraft acoustics and render measurements of acoustical efficiency, both for personnel and apparatus, a matter of extreme difficulty.

This paper has indicated almost entirely applications of acoustics to air defence. That the study of these problems has civilian application need not be pointed out, and while the Acoustical Section has devoted itself primarily to military problems, many aspects of its work are of pure scientific interest.

For this purpose we are indebted to the Department of Scientific and Industrial Research for special grants, and our work has been followed by the Physics Board of this department with sympathetic interest.

## Discussion

The Chairman, in opening the discussion, said that the Society was very much indebted to Major Tucker for his extraordinarily interesting and stimulating paper. He believed he was correct in saying that Major Tucker had been very much concerned with putting the subject of sound reception on a proper scientific basis. He had been wondering, when reference was made to the application of sound reception appliances in times of peace, whether there was anything by means of which it would be possible to assist the landing of aeroplanes in foggy or in very misty weather, this being, in his opinion, the most important thing to be dealt with from the point of view of civil aviation. With regard to the author's remarks as to Stewart's ratio of 3 to 100, he did not quite understand what was meant there. In one case his trumpets had magnified the sound three times, but he (the Chairman) could not understand the reference to the magnification of 100, and perhaps the author would explain that a little more. He did not know whether the author had read a very interesting book by the Rev. Bacon, the great balloonist. Several times Bacon referred to the transmission of sound over very great distances, and once when up in a balloon early one morning, at a height of 10,000 feet, he had heard a cock crow quite as plainly as if he were on the ground. He had given several instances of that kind of telephonic communication with the ground which it was difficult to explain. Another point which had particularly struck him (the Chairman) was with regard to sound detection at sea in a fog. His experience of being in a fog at sea was that it was extremely difficult to know where the sound came from, certainly without instruments.

Mr. J. L. Hodgson said the Society was extremely indebted to Major Tucker for his very lucid paper on Sound Detection. It opened one's eyes to realise that so much had been done, and so scientifically, on this intricate and complex subject. What struck one when one thought it over was its immense importance in connection with both aerial defence and aerial attack. The present tendency was to attack by night or to attack under cover of great smoke clouds, or at least to defend by means of smoke clouds, and the probability seemed to be that in the near future both aerial offence and defence would be based on sound rather than on sight. The defenders would be

[22] E. A. Milne, Sound Waves in the Atmosphere. Phil. Mag., July, 1921.

unable to see the attacking aeroplanes, and the attackers would be unable to see their objective, so that only by finding one's enemy by sound would it be possible to carry on aerial warfare at all. For instance, one might imagine a great defending aeroplane, carrying on its back a smaller one, whose propeller and engine had been specially designed so that they would be silent, and carrying an efficient sound detector. One could imagine the attacking planes coming over the Channel by night, making noises, but being quite invisible. The plane carrying the small sound-detecting plane would shut off its engines and volplane at intervals, and listen for attacking planes. As soon as it heard them it would release the small plane above it, which would be very light and fast, and which need carry only a small amount of petrol. We might imagine that the sound-detecting appliances would be so perfect that such a small fast aeroplane would be able to follow the attacking planes, and hunt them down and destroy them silently and invisibly. Another possibility, of course, was that defence might be brought about without the use of carrying planes. A plane or a helicopter might be stationed out at sea, communicating by means of resonators and wireless with some central place, where attacking planes were concentrated, and these might be released for the attack by some simple wireless transmission. That was one side of the development. The other side was that, in order to be able to attack effectively, we must develop silent fighting and bombing planes. The whole question of aerial offence and defence thus seemed to centre around these problems of sound detection and sound prevention. For this reason it seemed scarcely possible to over-estimate the importance of the work Major Tucker and his collaborators were doing. Our very national existence might come to depend upon the insight and energy which they brought to bear upon their work.

With regard to Fig. 6, if a trumpet magnified high pitch sounds so definitely as there shown, would it not be possible by setting *one* trumpet in the direction in which the high-pitched sounds were heard most clearly to ascertain the direction from which these sounds proceeded? The use of one trumpet and one observer in place of four trumpets and two observers would enable great simplifications to be made both in the methods of observing and in the methods of training the gun.

The lecturer's remarks on sound horizons showed how vital it was that the listening stations should be high in the air rather than on the ground. For this purpose a helicopter of suitable design might prove of immense use, as the slow-moving propellers of such a machine could be much more easily silenced than the rapidly-revolving propellers of an aeroplane.

Would it not be possible to replace the fixed concrete reflectors shown by large gasbags of suitable shape inflated with hydrogen, and reflecting the sound, collected by trumpets, from their inner wall? Such reflectors could be made very mobile. They could be made more effective than the concrete reflectors, since they could be made larger and be filled with hydrogen instead of air.

Major H. E. WIMPERIS said that with a subject so intricate as that of sound detection, hedged around as it was with difficulties due to the confidential nature of the work, it was remarkable that Major Tucker had been able to give so graphic an account of his work and in a way which had interested them so much. He had had the opportunity some time since, through the kindness of Major Tucker, of seeing his excellent laboratory, and he had formed a very high impression of the scientific nature of this work. There were one or two questions which he would like to ask. In one of his illustrations Major Tucker had shown two curves, and had pointed out that one represented the noise of the engine and the other the noise of the propeller. He (Major Wimperis) did not quite understand and would be glad to learn how his apparatus was able to distinguish between the two sounds. In another illustration Major Tucker had shown a rather puzzling record of the sounds made by two Vickers Vimy machines. They were flying

at the same time, and the sound records from both seemed to show no phase difference at all, but complete synchronism, which seemed to him to be improbable. The Chairman had referred to Bacon's observation that, at a height of 10,000ft., he had heard a cock crow as plainly as he could have done had he been on the ground. Ten thousand feet, however, was but two miles, and he had known cocks that could manage to make themselves heard for fully two miles (laughter). Continuing, Major Wimperis wondered what was going to be the outcome of work of this kind. We were using sound for the detection of aircraft. On the other hand, Major Tucker had said that one of the worst disadvantages in civil aviation, from the point of view of the passengers, was noise. But if we succeeded in eliminating that noise we were apparently in difficult circumstances with regard to military defence. It seemed to him not unlikely that the solution might be found on the following lines. We could silence the exhaust easily enough; we could silence the engine mechanism if sleeve valves were adopted, or something of that kind; but he did not think anybody had succeeded in silencing the propeller noise. If we could eliminate the first two in civil machines, and leave only the propeller noise, he believed the passenger's needs would largely be met. This would leave the propeller noise, and he was quite ready to credit that Major Tucker's apparatus would be capable of detecting it.

Wing-Commander T. R. CAVE-BROWNE-CAVE asked whether the ability to detect and locate aircraft was as good for a formation as for a single aeroplane. He should imagine that the noise of many machines in company would be extremely difficult to work on, but Major Tucker had given such remarkable instances of how the different noises could be sorted out that he had no doubt that he could deal with a formation.

On what days was sound reception most satisfactory; on clear days, with large temperature variations, or on cloudy days? He had gathered that sound reception was good during fog, but his individual experience agreed somewhat with that of the Chairman, that one was rather mystified as to the direction of sound in fog. Perhaps it was that on clear days one seldom troubled to fix the apparent direction of sounds and one did not realise what errors would then be made. In fog one wanted this assistance badly, and this very necessity might make judgment less reliable and impress errors and difficulties more strongly on the memory. The greatest range Major Tucker had referred to was some seven miles. He did not know whether Major Tucker considered greater ranges probable by improvement of apparatus.

More valves would give greater magnification. Were there difficulties in isolating very distant sounds or was what Major Tucker described as the acoustic horizon a physical phenomenon which definitely precluded the reception of sound from sources "below" that horizon? The point appeared one of very great importance in air defence.

Colonel V. C. RICHMOND said Major Tucker was extremely fortunate in having a laboratory in which he could deal with this very difficult branch of physics. Most physicists were just about able to get to the down-to-the-fork and drop-plate stage, and then had to branch away into pure mathematics in order to advance any further with their sound work, but Major Tucker had had the advantage of working on the big scale which was called for in connection with sound experiments. He would like to ask one or two questions, but, if they dealt with confidential matters, Major Tucker would no doubt refuse to answer them. The first was in connection with the function of trumpets, which was a very debatable point. There was a reference in the paper to somebody obtaining a certain magnification of sound with certain trumpets. He would like to know precisely what that meant. Did it mean that the sound received on a certain area of diaphragm was increased in amplitude; that the wave was increased in amplitude by adding a trumpet? The ordinary lay mind could under-

stand that, but he would like to ask Major Tucker if he had any theories on the reverse problem of how it was that a trumpet actually magnified a sound which was started at the narrow end. That was a puzzle to the majority of people, and one was particularly interested as to how far it was a function of the physical properties of the wall of the trumpet itself. He had formed the impression that it was very much dependent, not only on the form, but also on the elastic nature of the wall. Another point he would like to raise was how far methods of concentrating light had been tried with sound. He was thinking of lenses made of gas or some material with a refractive index different from that of the atmosphere, and also of diffraction as used in a zone plate, for instance. Finally, with regard to the question of ballooning, his experience was that sounds could be distinguished in a most remarkable manner when in a free balloon very high up. The chief impression he had in free ballooning, especially on the first trip, was the extraordinary way in which one could analyse the sounds heard when passing over a town. He would like to know whether any physical reason had been discovered so far as to why the power of analysis should be greater at a height than when near the ground.

Commander CAVE thought that the good hearing when near a balloon was due to a lens effect of hydrogen in concentrating the sound, but he would like Major Tucker to confirm this.

The CHAIRMAN pointed out that the balloonist was suspended below the balloon itself. If he were sitting on the top of the balloon he might get a lens effect from the ground, but he could not see how that could be the case when the balloonist was beneath the balloon.

Commander CAVE replied that surely it was a combination of reflection and lens effect.

The CHAIRMAN: Reflection from the inner top surface?

Commander CAVE: I think so.

Major TUCKER, replying to the Chairman's remarks as to the direction of sound in fog, said that we did get, on certain days, a long-drawn-out reverberation. Experiments were being carried out in that connection. The results were not exactly what were expected, but seemed to lead to our being able to anticipate that, with the signals of sufficiently long duration, we might get at the source of sound. But there were occasions when the atmosphere was foggy and we did not get reverberation, and there must be also a number of cases where we could get perfect acoustic conditions. He could only say, from experience of locating guns, that it was most remarkable how clear the records of a gun explosion—which was the very thing for producing reverberations—were during a fog, on the microphone apparatus, as a rule. In a large percentage of cases he would declare in favour of a fog for getting direction. In any case, there were days on which we could not rely on location, but there were other days on which we could.

With regard to ranges, he had only shown some records taken at a range of seven miles. In a lecture which he had given in the same room some time ago he had given records at nineteen miles; they were quite definite, and quite easily identified. Of course, he wanted to push the limits of sensitivity as far as possible, and our apparatus was getting more and more sensitive. We were making use of wireless apparatus, which was developing so extraordinarily well, in conjunction with the best that could be produced in the way of microphones, in order to get long range. But there were other difficulties. There was the acoustic horizon limitation, which came about under adverse conditions, and there was a definite coming in and going out of the sound. We could calculate elevation, and say that a plane would rise over the horizon at a certain moment, but there were conditions when the air was full of acoustic clouds. We could hear a plane

coming along before it rose above the horizon, just as we could get the advent of light before the sun rose or after it had set. With favourable listening conditions there was no limit beyond which we could hear an aeroplane, or anything else, provided the apparatus used was sufficiently sensitive. The difficulties to be contended with in regard to range were due more frequently to adverse listening conditions rather than to the failure of the instrument.

As regards the discrepancy between the two magnifying powers quoted by Stewart—the points raised by the Chairman—the magnification of 100 was obtained by a purely local short range experiment, that of 3 by observation with an aeroplane at long range. Magnification figures were probably obtained by applying the inverse square law working on ranges at which the sound just became inaudible. The large discrepancy was due to losses in the atmosphere.

The exact explanation of the magnification of trumpet as a receiver of sound is probably not known. Both resonance and what may be called the collecting power of the trumpet play a part in this phenomenon.

If the trumpet is used as a transmitting agency as in the loud speaker, it serves as a medium for conveying vibrational energy to the air—in other words, it improves the efficiency of the process of converting the mechanical vibration in a diaphragm into sound waves.

With regard to the analysis of sound when in a balloon, he had carried out experiments with hydrogen, and had found that there was extraordinarily good reflection from a balloon surface, especially if it were concave, because of the extremely low refractive index of hydrogen. As Commander Cave-Browne-Cave had pointed out, the sound would pass from the ground to the upper surface. There was reflection from the upper surface, and it would be focussed below the balloon. He considered that, undoubtedly, was the explanation.

In answer to Major Wimperis, separation of exhaust and propeller sound, as shown in the records, is effected by the use of separate tuned microphones.

With regard to the record of two Vickers-Vimy machines, Fig. 15 shows a succession of deep waves whose outlines are themselves carrying a slight ripple.

It is known that for the short time interval represented in the record, two engines in one plane were synchronous The contribution of this machine to the total sound is shown in a steady displacement of the axis of the curve.

The large waves are due to beats between the engines of the other machine equivalent to a difference in revolutions of twenty per minute.

This second machine is out of tune with the first one by an average of sixty revolutions per minute, or one beat a second, and this beat is indicated in the ripple on the record.

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A hearty vote of thanks was accorded Major Tucker at the conclusion of the discussion.

# PROCEEDINGS

## ELEVENTH MEETING, 59TH SESSION

A meeting of the Royal Aeronautical Society was held in the Rooms of the Royal Society of Arts, John Street, Adelphi, London, on Thursday, March 20th, 1924. Colonel A. Ogilvie, Chairman of the Society, presided.

The CHAIRMAN, in calling upon Mr. W. S. Farren, M.B.E., to read his paper on "The Work of the Aeronautical Research Committee's Panel on Scale Effect," said it was well known that Mr. Farren's work on scale effect had been extremely valuable. Mr. Farren had been Chairman of the Design Panel of the Aeronautical Research Committee, which had been working on the question of scale effect for the last two years or so. Prior to that he was at the Royal Aircraft Establishment in charge of all the aerodynamic work, and had done a great deal of piloting on aeroplanes and seaplanes.

Mr. FARREN then read his paper.

## THE WORK OF THE AERONAUTICAL RESEARCH COMMITTEE'S PANEL ON SCALE EFFECT

### 1 *Introduction*

If I were to take my title literally, my task would be light, and this paper would end here. For there is not, nor has there been for some six years, a Panel of the Aeronautical Research Committee on Scale Effect. It would be unfair to cast the responsibility for this error on our Secretary, as I could have changed the title when I replied to his letter asking me to read a paper before you. But as it did not seriously misrepresent the facts, and was less cumbersome than any alternative I could think of, I let it stand.

The Panel to whose work I am permitted to draw your attention is the Design Panel of the Aerodynamics Sub-Committee. It has been my privilege to be Chairman of this Panel for the last year or more. It may not be out of place to mention that the justification for its title is that it deals with the part of the Sub-Committee's work which most directly concerns the design of aeroplanes. In addition to aerodynamics, it deals with the statics of aeroplane structures, but it does not touch stability and control.

As evidence that the Panel is not out of touch with those whom it endeavours to help, among its members are Captain Barnwell and Mr. North, while Colonel O'Gorman and the Chairman of our Society keep a watchful eye on such of its activities as might verge on matters political. The N.P.L. and R.A.E. are strongly represented. Mr. Southwell and Mr. Wood were my predecessors in office. Mr. Relf also is a member, and Mr. Stevens and Mr. Glauert take active parts in the Panel's work. We have also the assistance of Professor Pippard, Mr. Howard of the Airworthiness Department, and Professor Levy.

The Design Panel has been working for some two years with the idea of summarising the present state of knowledge of scale effect on the principal characteristics of aeroplanes—lift, drag, and centre of pressure. We believed that designers would welcome a reasoned statement, embodying all the most reliable

experimental results, and such general conclusions as could be drawn. In order to clear up points of difficulty, permission was obtained for supplementary experiments to be made at the R.A.E., and it is on account of these that the production of the report has been delayed.

The report is now in the hands of the Aeronautical Research Committee. As it has not yet been passed for publication, I cannot quote from it, but I have the Committee's permission to make use of the experimental results. For this I must here express my thanks. I must also make it clear that the opinions I express are purely personal, and do not carry the authority either of the Committee or of the Design Panel. The report itself will be published by the Stationery Office as soon as it has received the Committee's approval.

I cannot conclude this introduction without reminding you that none of the credit for obtaining the experimental results I shall show you is due to me, but all to the staff of the R.A.E. and N.P.L. I do not think that Mr. Southwell and Mr. Relf will feel that I am unjust to them if I say it is mainly due to Mr. Wood and his colleagues at the R.A.E. It was my privilege to occupy the position Mr. Wood now fills so ably, for the three years ending May, 1918, and I can therefore speak from personal knowledge of what their work demands. But I cannot claim a share in it. How exacting it is, what perseverance it needs, you may judge from the fact that some of the work, whose results were published in April, 1923, was begun before I left the R.A.E. If I hesitate to mention individual names, it is because I could not with justice omit anyone.

Of the flying staff of the R.A.E. I must be allowed to say one word. This Society, I am sure, feels a sense of personal loss in the transfer (I had almost written translation) of Major Roderic Hill to other work—doubtless necessary, in this imperfect world. We know how much he did to infuse into his staff the spirit of research, and we are confident that this will inspire his successor. Without their sympathy the work cannot be done. Much of the advance that has been made in some of the most difficult experiments is due directly to their skill and patience.

## 2 *What the Designer Wants*

The paper which Mr. Wood read before you on November 16th, 1922, dealt with aspects of the scale effect problem, on which I shall not touch. I propose to regard it mainly from the point of view of the aeroplane designer and to examine how far the work I describe answers his question: "How far can I trust model experiments to predict the characteristics of aeroplanes?" For that, in my view, is what he really wishes to know.

The essential difficulty of aeroplane design is the smallness of the margin between success and failure. A pessimistic bridge designer will produce a heavy and expensive bridge, but it will probably be a safe bridge. It may even be judged a sound conservative structure, and the particular Board which approves bridges (I suppose there is one) will mark it A.1. But an aeroplane built on similar lines will be a failure. It may even fail to perform its most elementary function—to fly. It is lightness at which all aircraft design aims. Anything which saves weight enables aeroplanes to be made safer.

I do not wish to be misunderstood. A certain conservatism in design is necessary, but it should be firmly based on knowledge. In the long run, ignorance of fundamental aerodynamic characteristics can only result in superfluous weight.

The information which the designer requires is mainly of one of two kinds:—

(1) General information on such problems as the effect of variations in aspect ratio, and the interference of the body with the wings.

(2) Particular information concerning special features of his own design, such as the resistance of an undercarriage.

The former he needs to guide him in deciding the general outline of his design, the latter to arrange the details. It is generally agreed that he must make his own arrangements for obtaining what I have described as particular information. Whether they should involve the erection of a private wind tunnel cannot, in my opinion, be decided without careful examination of many circumstances.

In the past the designer has looked to the Government establishments for authoritative general information. The work on scale effect is testimony to the fact that these establishments have a full sense of their responsibility. For they could not, in my view, properly adopt the attitude " Here are our measurements of the characteristics you require, made on a model. The experimental accuracy is as good as anything you will get anywhere. But we do not advance them as applicable to the full scale. We believe they are, but that is not our job." To do this would be to forget that the information is issued by the authority of the Aeronautical Research Committee, and while that does not of itself imply that the figures are applicable to the full scale, it certainly does convey to the aeroplane designer the idea that deductions made from them, intelligently, are not likely to be misleading.

I wish to observe that I am referring here solely to measurements made specifically for the assistance of aeroplane design, and not to those concerned with what I must call, for want of a better term, fundamental research. I take it we are all agreed that the latter deserves every encouragement and assistance we can give it. This Society has given practical proof that it believes this, and it appears that its efforts may be rewarded. I believe that every aeroplane designer will support the course it has taken.

It is important that the designer should make up his mind on the question what are the errors he is prepared to put up with. For the problem is one of measurement, not of calculation. His answer will probably be that he does not require great accuracy in the absolute values of characteristics, except on occasion, but he does desire to be assured that the figures supplied to him indicate closely the relative values of the full scale characteristics. For his main anxiety is to improve his aeroplanes.

This is not really a logical requirement. If it is suspected that a set of measurements are subject to uncertainty in their absolute values, only a thorough knowledge of the laws they obey can justify reliance on their relative values, unless the differences between them are large compared with the suspected discrepancy.

Apart from what the designer *needs*, he *hopes* for something which he has really little right to expect, that outline models—rough mock-ups of bodies attached to hardly less rough models of wings—will throw light on the characteristics of aeroplanes. Further, that typical characteristics of wing sections, bodies, and so on, may be determined, to be combined at will by little more than simple addition. Knowing what we do of the complexity of the problem of fluid flow, I think we could only regard such a state of affairs as due to a direct dispensation of providence. In a science whose laws are relatively simple, such as electricity, we do indeed find such a dispensation, but I am bound to confess that aeronautics does not seem to me to be similarly blessed.

## 3 *The Problem*

I do not propose to state the general problem of scale effect, as it has been stated on many occasions by others more able than I. But it is, I think, useful to remark that it is a problem that appears in various forms in many branches of engineering.

In diagrams which show model and full scale tests on a water turbine, and show its performance and its efficiency under various conditions, it is obvious from the general shape of the curves that such differences as exist are not of a fundamental nature, though there is by no means complete agreement.

## 4 *History*

The particular problem of scale effect, in which we are concerned, first arose in 1915 in a rather interesting way. An inquiry came back to the R.A.E. from the front asking why the top speed of aeroplanes fell off with height. At this date it may seem rather remarkable that such a question should have been asked, and indeed to us it appeared at first as though those from whom it originated had not realised that the reading of an air-speed indicator falls, as the density of the air decreases, for the same true speed. There was, however, a real basis for the inquiry, and it resulted in attention being directed to the variation of the performance of an aeroplane with height. It must be remembered that in 1915 climb tests of aeroplanes seldom reached 6,000ft.

Let us consider a simple problem of this nature, in which the resistance of the body concerned varies directly as the density of the air and the square of the speed. If we assume that the power of the engine at any fixed number of revolutions per minute is proportional to the atmospheric density, and that the efficiency of the airscrew is a function of $V/N$, then it can be seen without difficulty that the true speed maintained is independent of the density, that is, of the height. This is realised approximately by an airship. A difference might be expected in the case of an aeroplane since the resistance is not proportional to $\rho V^2$. For, if the weight is constant, a constant speed and a decrease in density implies an increase of angle of incidence, and therefore, in general, of drag coefficient. If, however, at top speed at ground level the aeroplane is flying near to the angle at which the drag is a minimum, we may expect the speed to fall off comparatively slowly, if at all, with height.

Experimentally it was found that the speed fell off fairly rapidly with height, and as it was believed that the assumptions concerning the engine and airscrew were not likely to be far in error, it was suspected that the variation of aeroplane resistance with angle of incidence was not what the model tests indicated.

From this starting point the inquiry developed rapidly and full scale experiments were made on a number of different aeroplanes with the same wing section. Using the best available information as to the engine's power and the efficiency of the airscrew, the results were expressed in the form of a curve of drag coefficient of the wings against lift coefficient. The fact that the experiments were made on several types of aeroplanes necessitated dealing with the results in this way, and the agreement obtained was encouraging. Nevertheless, no great trust was placed in the absolute values deduced, attention being confined to the problem from which the research arose, which corresponded to the slope of the curve of drag coefficient against lift coefficient in the neighbourhood of the top speed of the aeroplanes.

Differences appeared here which gave rise to a feeling of uncertainty, and it was thought advisable to try another kind of investigation. Mr. G. I. Taylor undertook the measurement of distribution of pressure over one rib of an aeroplane wing, and developed a photographic method which was the basis of the one now used.

Experiments were made on the wing section known as "old B.E.2c," which in some respects bears a resemblance to another section on which I shall have some remarks to make later. Although there was by no means complete agreement between the model and full scale pressure distribution, nevertheless there was little sign of a difference of the kind suspected.

Shortly afterwards the Advisory Committee for Aeronautics appointed a sub-committee to deal with the problem, and at its suggestion work was confined to one aeroplane. It was decided to compare the resistance of the complete aeroplane and the corresponding model, no attempt being made to separate the resistance of the various parts. Pressure distribution experiments were also made, the aeroplane having R.A.F.14 wing section.

The agreement between the absolute resistance coefficients of the model and full scale was very much better than on the preliminary investigation to which I have referred, but it was not complete and no light was thrown on the main discrepancy. Remarks were made at the end of the report emphasising the need for further research.*

The general agreement of the pressure distribution experiments was all that could be expected, with the exception of the pressure over one surface of one wing, in which there was a consistent difference.

It is interesting to note that the two authorities who have used the results of this experiment as decisive evidence on the problem of scale effect have drawn very different conclusions from them.

## 5 *Plan Underlying the Work from* 1918-1924

It was evident that no further progress could be made until the number of uncertainties in the full scale experiments could be reduced. These lay chiefly in the engine power, the airscrew efficiency, and the interference of the airscrew slipstream. All of them could be eliminated by making the experiments when gliding with the airscrew stopped. This entailed an appreciable increase in the time involved in an experiment, but the great gain in the simplicity of the comparison with the model experiments outweighed all objections of this kind. There is a more serious objection—that we really want the characteristics of the aeroplane in ordinary flight and not when gliding with the airscrew stopped. This can only be met by the remark that it is better to get reliable information in stages than to attempt to deal with the whole of a very complex problem at once.

It was decided that no analysis of the resistance of the full scale aeroplane should be attempted, and that as far as possible measured quantities should be compared directly.

It is important to notice that there are certain features of model experiments which make it misleading in general to compare the actual measured resistance of the model with any full scale measurement. It is not practicable to reproduce on the model all the features of the aeroplane, and while the corrections which have to be applied on this account are small, they are by no means negligible. There are also certain features on which there is known to be a large scale effect, and it is on the whole desirable to make allowance for this before the results are compared.

The chief objection to the original comparison which was made between the model and the full scale characteristics was based on the belief that the accuracy of the model experiments was very much higher than that of the full scale work, and that until the full scale accuracy could be improved, no substantial objection could be entertained to the applicability of the model characteristics. I have always found it a little difficult to accept this contention in the form in which it was made, as it did not seem to me that there was really a large probable error in the full scale measurements. Undoubtedly certain characteristics could not be determined with the same accuracy as on the model, but over the most important range I have never believed that the error of the full scale experiments was much more than could be attributed to atmospheric disturbances. In support

* R. & M. 374, Annual Report A.C.A., 1917-18, Vol. II., p. 535.

of this view I draw your attention to Fig. 1, where the scattering of the results obtained by three methods are compared graphically. Nevertheless, it must be admitted that many somewhat uncertain corrections were involved, and it was mainly on this ground that the policy described above was adopted.

As regards the accuracy of the model experiments there was little fault to find. But it has to be realised that hitherto there has been no direct evidence which would justify us in maintaining that the systematic errors in any one experiment were negligible. It is true that it is difficult to suggest reasons why there should be any systematic errors. You are all familiar with the precautions which are taken to exclude them in all wind tunnel work. Nevertheless, everyone who has done any experimental work will readily admit that, in spite of every precaution, systematic errors creep in. The best way to investigate their magnitude is to repeat the experiment—in this case in a different wind tunnel. This cannot be done regularly for obvious reasons. Recently, however, experiments of this kind have been made on a single aerofoil of R.A.F. 15 section in two wind tunnels at the R.A.E., and two at the N.P.L. All the circumstances of the tests were very carefully considered, and it may be taken that the differences disclosed

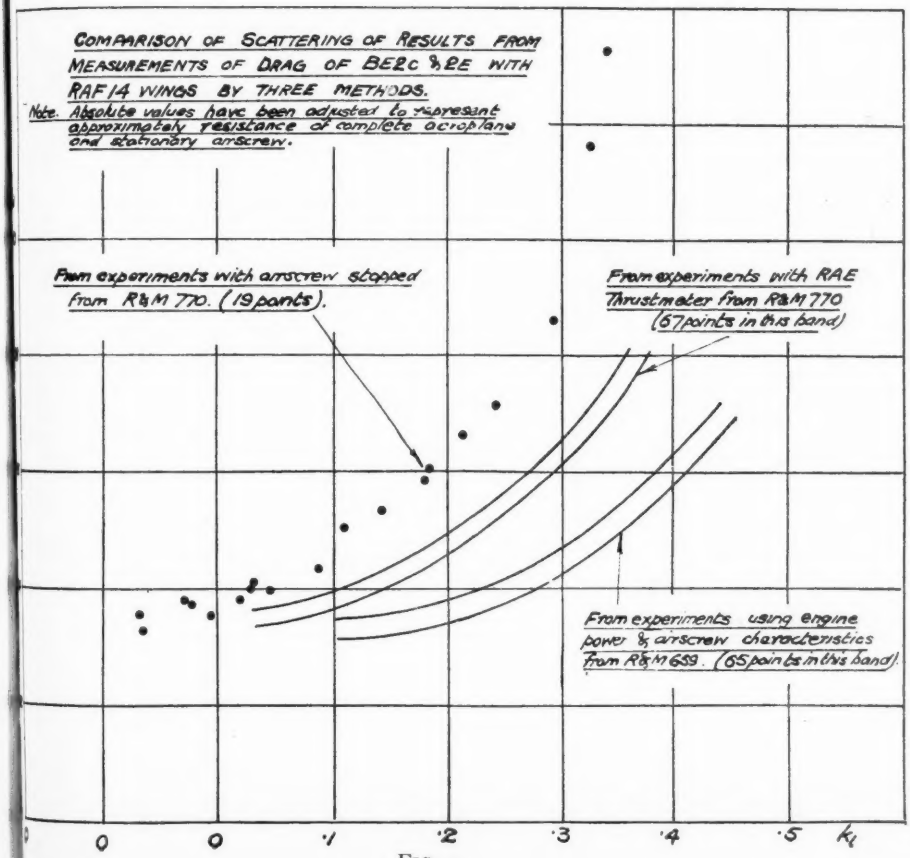

COMPARISON OF SCATTERING OF RESULTS FROM MEASUREMENTS OF DRAG OF BE2c & BE WITH RAF 14 WINGS BY THREE METHODS.
Note. Absolute values have been adjusted to represent approximately resistance of complete aeroplane and stationary airscrew.
From experiments with airscrew stopped from R&M 770. (19 points).
From experiments with RAE Thrustmeter from R&M 770 (67 points in this band)
From experiments using engine power & airscrew characteristics from R&M 659. (65 points in this band).
0
0
·1
·2
·3
·4
·5
$k_1$

FIG. 1.

are due to causes of which account is not usually taken in wind tunnel experiments. The results of these experiments are shown in Figs. 2, 3 and 4. I have not attempted to distinguish between the results from the various wind tunnels. As a matter of fact, those from each wind tunnel lie on a smooth curve. When, however, the results are plotted on one diagram you will see that there is an appreciable scattering. It is presumed that this corresponds principally to a number of slight departures from true similarity of all the experimental conditions.

In each of Figs. 2, 3 and 4 there is another curve showing a measurement of corresponding quantities on the full scale. The absolute magnitudes of the model and full scale quantities in these diagrams do not bear any relation to one another.

You will notice that there is no striking difference in the order of magnitude of the probable error in the two classes of experiment. This, if admitted, makes the comparison of model and full scale results at once easier and more difficult—easier, because we are no longer open to the objection that the comparison is between results of two entirely different orders of accuracy; more difficult, because in all the model experiments with which comparison is made, no repetition of the kind shown in Figs. 2, 3 and 4 can be made, so that we have in every case only one model experiment with which to compare the full scale results.

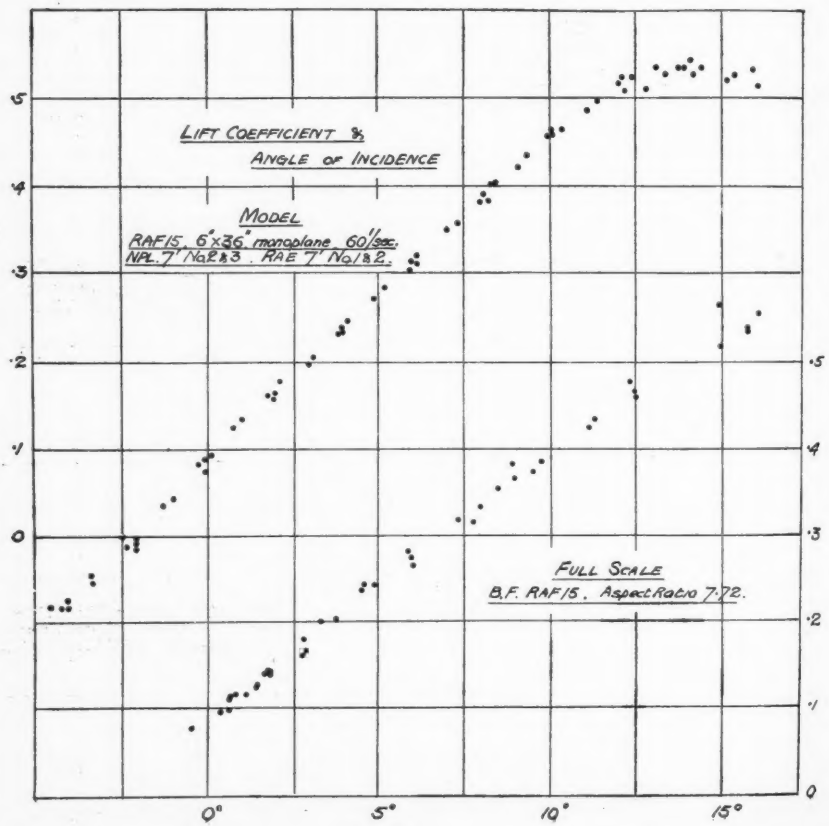



FIG 2.

The scattering of the full scale results is probably due mainly to up and down currents in the air. No attempt is made to measure these, and it is realised that unless the full scale experiments are repeated, preferably several times, no reliance can be placed on their results. All the full scale results I shall show you, therefore, contain this scattering, whereas the model results do not. It might be considered by some that a fairer comparison could be made by drawing the best mean curve through the full scale results, and comparing that with the model. This course has not been adopted, mainly because it is thought that it is valuable to have on every diagram an indication of the order of accuracy.

We have finally to make allowance for the main feature in which the model and full scale experiments are not geometrically similar, which is due to the presence of the wind tunnel walls in the model work. I do not propose to enter

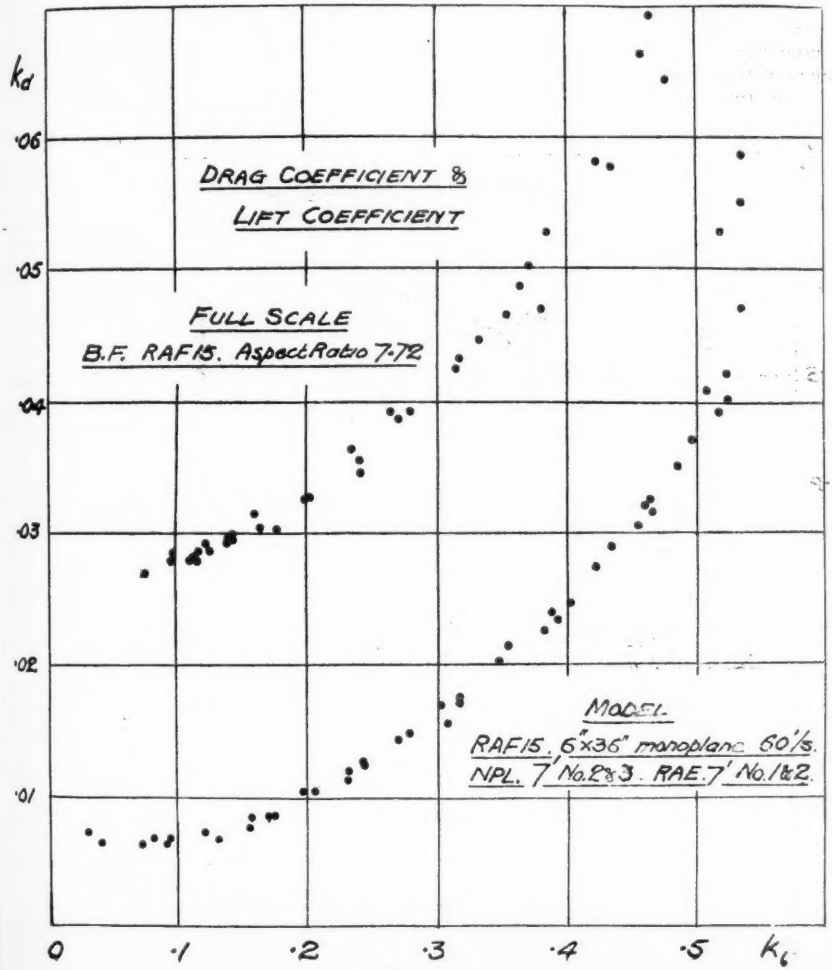



FIG 3.

into a discussion of the Prandtl theory, but you are aware that it enables us to estimate the effect of the tunnel walls on the characteristics of an aerofoil, and that exhaustive experiments have shown that the correction does bring into agreement tests on the same model in wind tunnels of various sizes. The correction has been applied to all the results shown below, and you will be able to judge the extent to which it improves the agreement from Fig. 5, which shows the comparison between one set of full scale experiments and the corresponding model results with and without the correction.

## 6 *Results*

The first set of results shown in Figs. 6 and 7 are those referred to by Mr. Wood in his paper a year ago as being then incomplete. They were made on a Bristol Fighter aeroplane with wings of three different aspect ratios. You will observe that the agreement on lift is almost complete, including now the maximum lift which has hitherto been rather uncertain. Owing to an improvement which has been made in the full scale method of measuring air speed, and also to the experience which the pilots now have in flying steadily in the neigh-

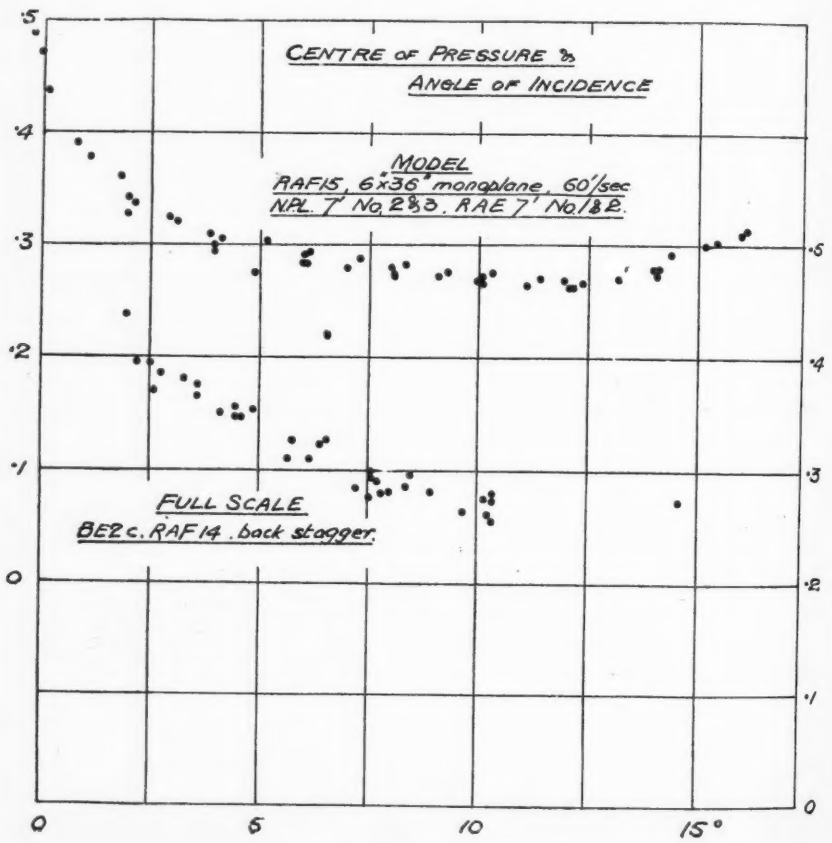



FIG. 4.

bourhood of the stall, the accuracy of the full scale determination is considered to be practically as good as at higher speeds.

In drag there is a fairly uniform discrepancy. The full scale results agree with the shape of the model curve, but are generally below it by an amount which is considered to be greater than the probable error of the experiments.

A difference of this kind suggests that there is a scale effect upon some part whose resistance does not vary much with angle of incidence. It is impossible, of course, to do more than suggest what may be the real cause, but from further results given below it would seem probable that it is due to the body or stationary airscrew, or to their combined interference with the wings.

The next diagram, Fig. 8, shows results of experiments on S.E.5 with two shapes of body and two arrangements of wing structure. The standard machine agrees completely with the model. These results are in fact Fig. 8 of Mr. Wood's paper. On the other hand, with a different body and the same wings, there is a large discrepancy, the reduction of resistance predicted by the model not being realised on the full scale. The third set of results representing the remaining

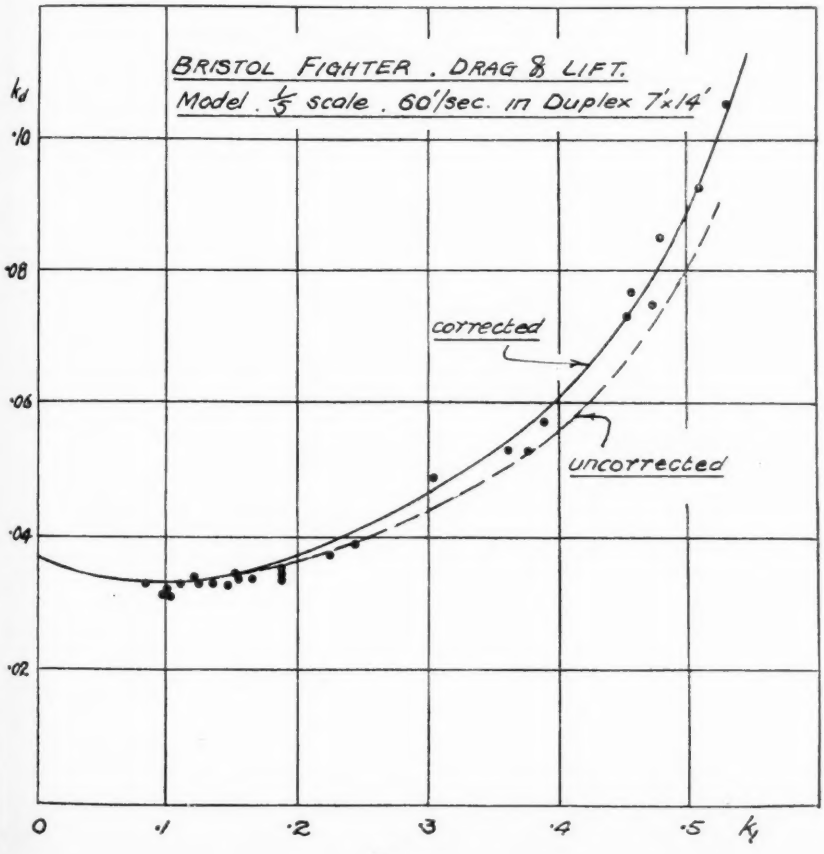



FIG. 5.

combination of wings and body suggests that the discrepancy is not due to the wings.

It may be noted that full scale experiments have also been made on these aeroplanes with the airscrew running. It is not easy to draw clear conclusions from a comparison of the results with those shown in Fig. 8, since the radiator associated with the streamline body was exposed, whereas it was withdrawn on the glides, but the broad result that there was hardly any difference between the

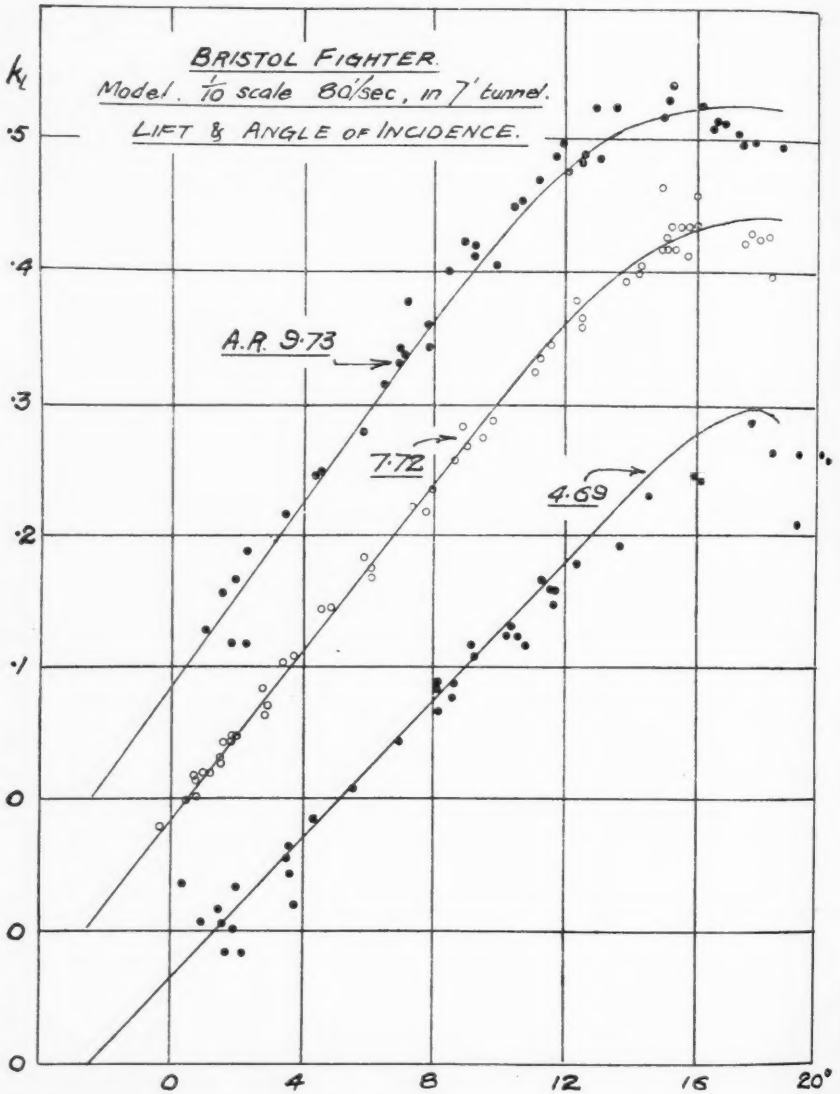



FIG. 6.

performance of the two full scale aeroplanes, with the airscrew running, is not without interest.

When I last had the privilege of reading a paper before you I described the method by which it was hoped to determine the centre of pressure of the full scale wings by measuring the moment of the tail and rear part of the body about a hinge which was made in the body immediately behind the pilot. The results of

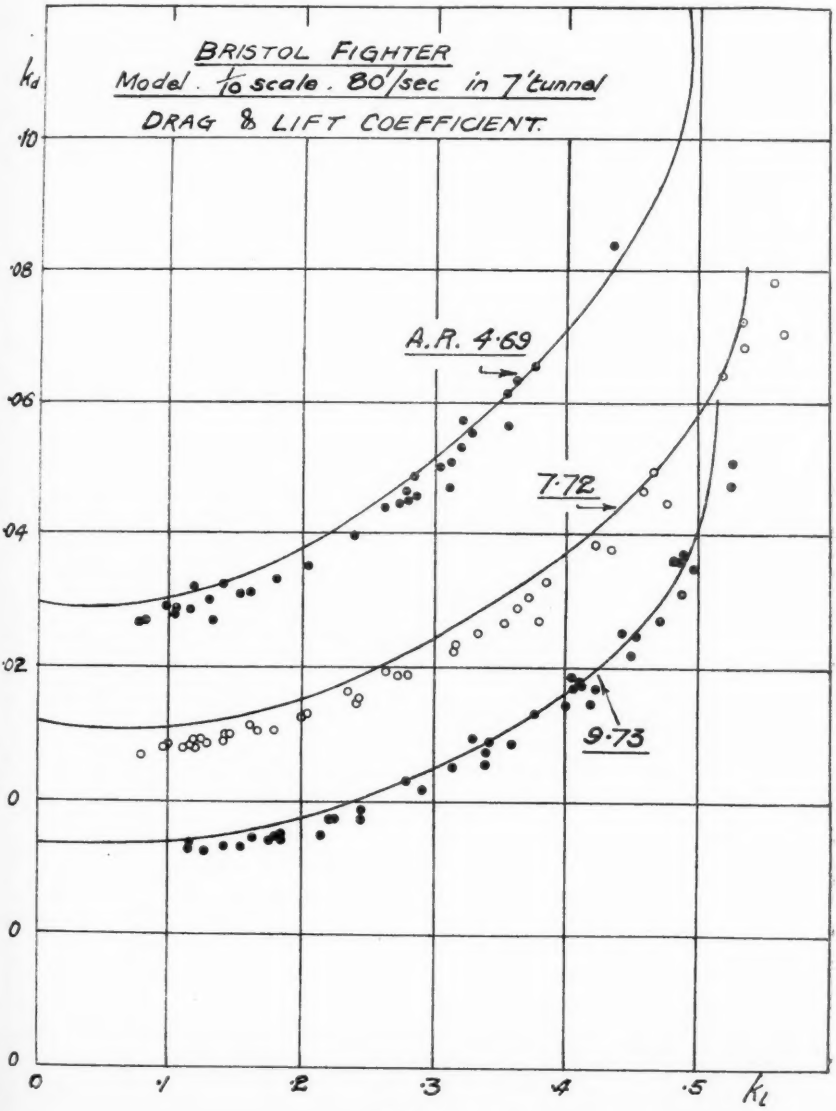



FIG. 7.

experiments of this kind have been published from time to time during the last six years, and have all shown a fairly consistent discrepancy between the model and the full scale. The most recent experiments, also made with the airscrew stopped, are shown in the next diagrams, Fig. 9, and refer to a B.E.2c aeroplane with wings of three different staggers. You will see that the discrepancies persist and are fairly uniform.

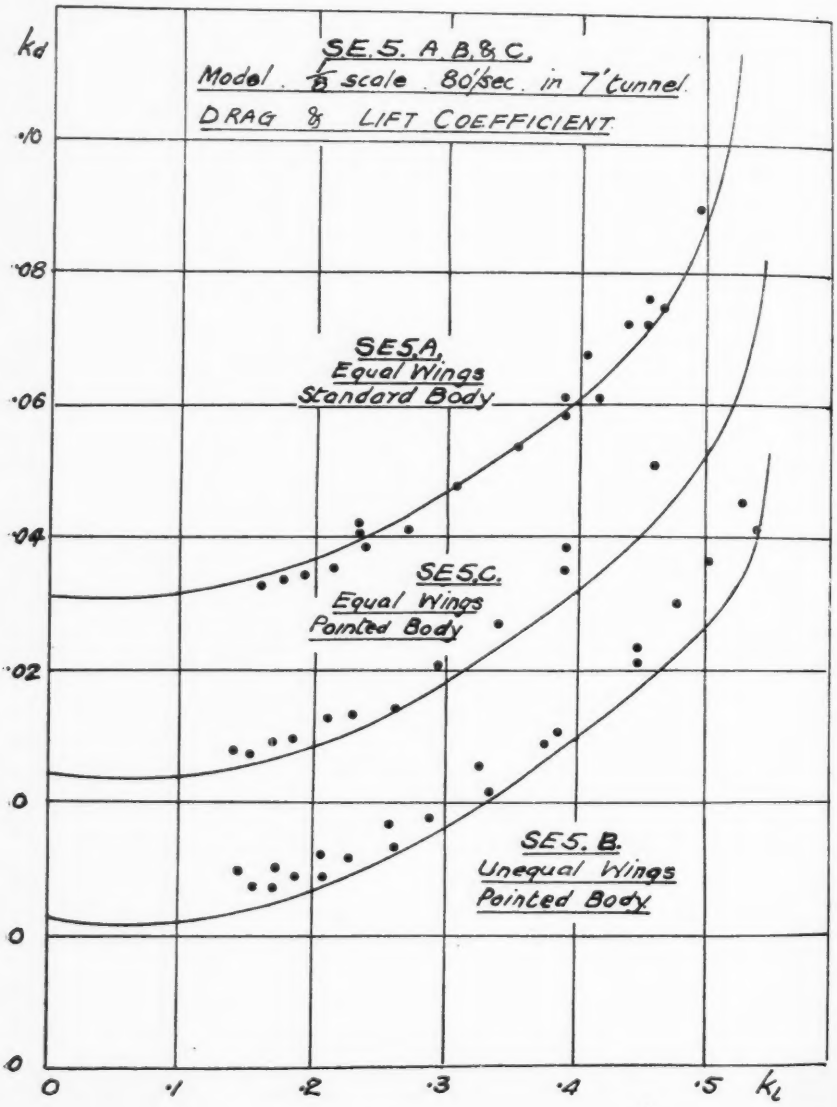



FIG. 8.

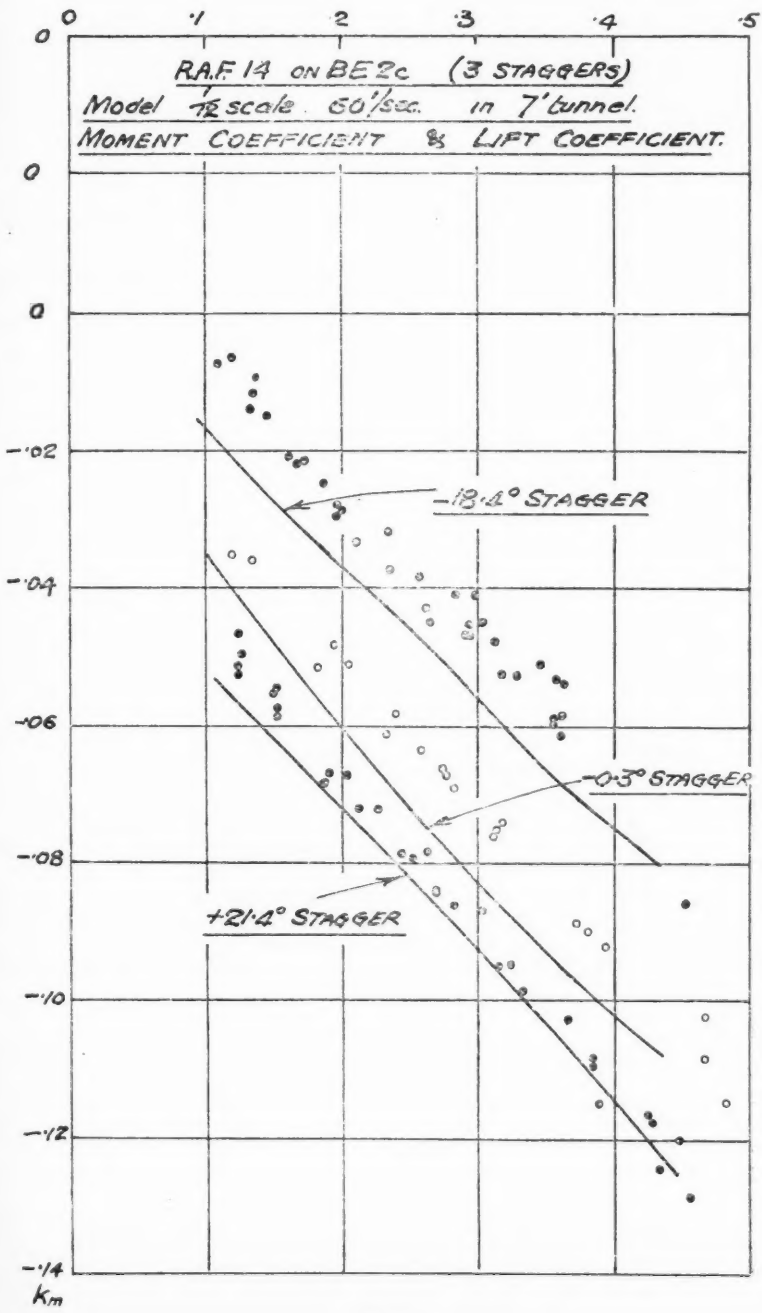

R.A.F. 14 on B.E.2c (3 staggers)
Model 1/12 scale. 60'/sec. in 7' tunnel.
Moment Coefficient & Lift Coefficient.
0
·1
·2
·3
·4
·5 $k_L$
0
0
-·02
-·04
-·06
-·08
-·10
-·12
-·14
$k_m$
-18·4° stagger
-0·3° stagger
+21·4° stagger

FIG. 9.

A great deal of argument has centred around these experiments, and it is admitted that their accuracy is not as good as that of the others I have shown you. Nevertheless, it has not been found possible to attribute to the full scale experiments a probable error of more than about one-third of the difference between the model and the full scale, so that their evidence cannot be rejected at the moment. In view of what I have shown you, and of the results of the pressure distribution to which I refer next, it seems probable, however, that these results are really due to an effect originating where we have already reason to suspect a difference, namely, in the body or its interference. It is realised that it is important to investigate this discrepancy further.

The last results from aeroplanes with wings, of what I may describe as a normal type, are the pressure distribution experiments on R.A.F.15, Fig. 10.

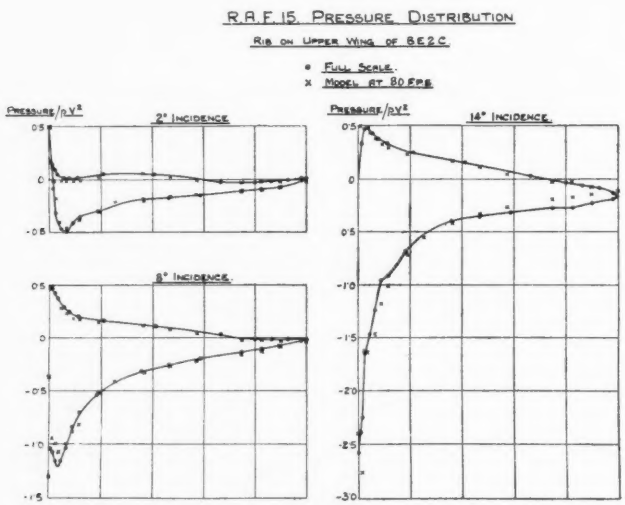



FIG. 10.

These show good agreement between model and full scale, though on integration (which might be expected to level out small differences) there is a 5 per cent. difference in normal force at all angles. In view of the agreement which we have seen has been found between the model and full scale lift, it would appear that the discrepancies in these results are really rather a measure of the accuracy which can be attained in pressure exploration than signs of a real difference.

The above diagrams represent fairly the results of all the tests on aeroplanes fitted with normal wing sections, and I think that we can deduce from them that it is improbable that there is any appreciable scale effect on the wings. Moreover, if suitable precautions are taken to correct for known disturbances, model tests on such aeroplanes will predict the differences between the characteristics of various arrangements of wing structure with all the accuracy we need for design purposes.

On the other hand, it appears that a similar deduction cannot be made when variations of the shape of the body of the aeroplane are in question. Whether this conclusion applies when the airscrew is running, I cannot say. Model experiments with the airscrew running are now in progress, and results should be available very soon. Until they have been analysed I think it is best to be very

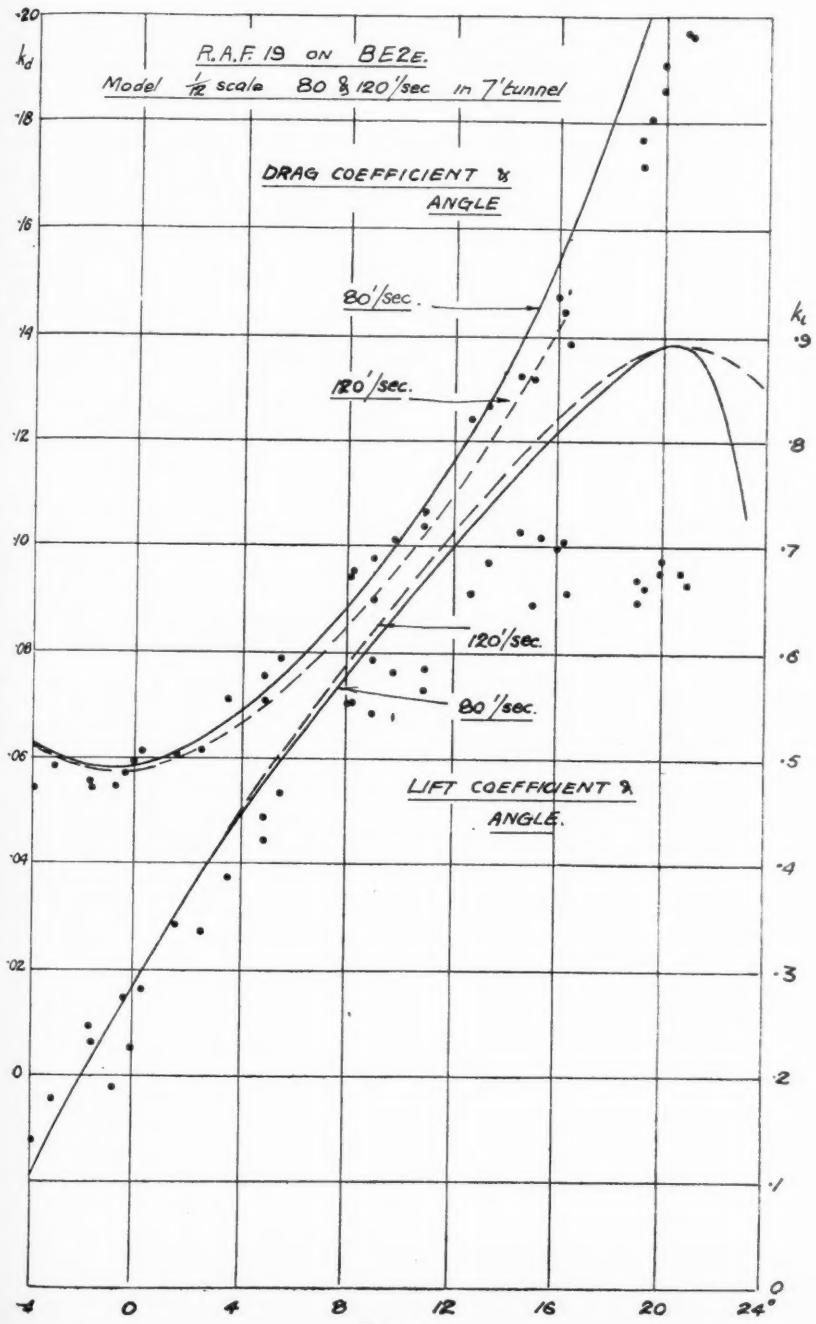

R.A.F. 19 ON BE2E.
Model 1/12 scale 80 & 120'/sec in 7' tunnel
DRAG COEFFICIENT & ANGLE
80'/sec.
120'/sec.
120'/sec.
80'/sec.
LIFT COEFFICIENT & ANGLE.
$k_d$
$k_l$
·20
·18
·16
·14
·12
·10
·08
·06
·04
·02
0
·9
·8
·7
·6
·5
·4
·3
·2
·1
0
-4
0
4
8
12
16
20
24°

FIG. 11.

cautious in applying model tests of bodies to the full scale. It is probable that the position of the centre of pressure of the wings is accurately given by the model, but the full scale characteristics, as presented, are likely to be a better guide than the model in calculations of tail setting.

I have finally to show you some results in which the scale effect is large. Some of them you have seen before. They were obtained on an aeroplane fitted with R.A.F.19 wing section and are shown in Figs. 11 and 12. R.A.F.19 is of no direct interest to aeroplane designers, though it is important to realise that its shape is not as extreme as is sometimes supposed, except when regarded as suitable for flight at small angles of incidence. Scale effect of this kind has been observed in tests made in Germany, the results at low speeds being above those

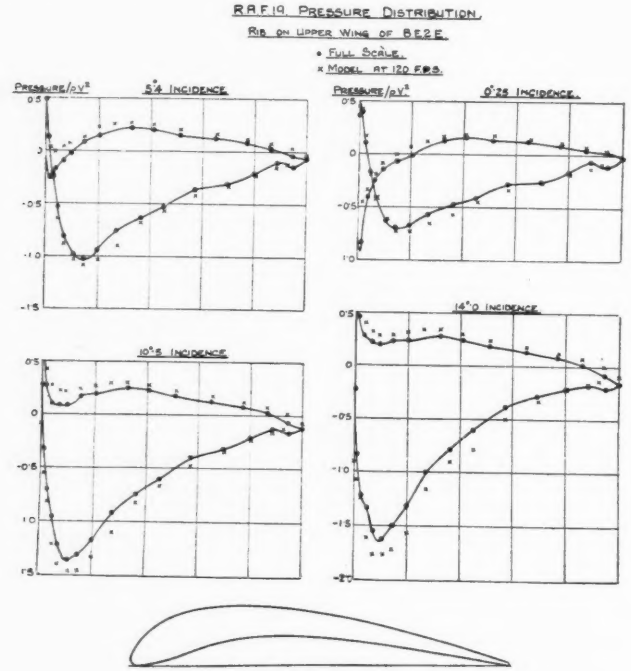



FIG. 12.

at high speeds for some sections, and below for others. The importance of the results to us lies in this—that until we know what is the characteristic of the aerofoil to which they are mainly due, we cannot regard the agreement between model and full scale tests of aeroplanes fitted with normal wing sections as applicable to those with what are commonly known as high lift wings.

## 7 *Conclusions*

I summarise my own conclusions from the results of this work as follows:—

(1) I accept the results as accurate to the order indicated by the scattering in Figs. 2, 3 and 4. It is difficult to give numerical expressions to these. One may perhaps say that at a given angle any one measurement of lift

coefficient on a model is subject to a probable error of $\pm 0.01$, and that at a given lift coefficient the drag coefficient is subject to a probable error of $\pm 0.001$. The diagrams suggest slightly larger probable errors on the full scale than on the model. I do not think the error can properly be expressed as a percentage.

(2) The lift coefficient of an aeroplane of normal type, fitted with wings similar to R.A.F.15 in section, agrees with that of the corresponding model over the whole range of angle of incidence.

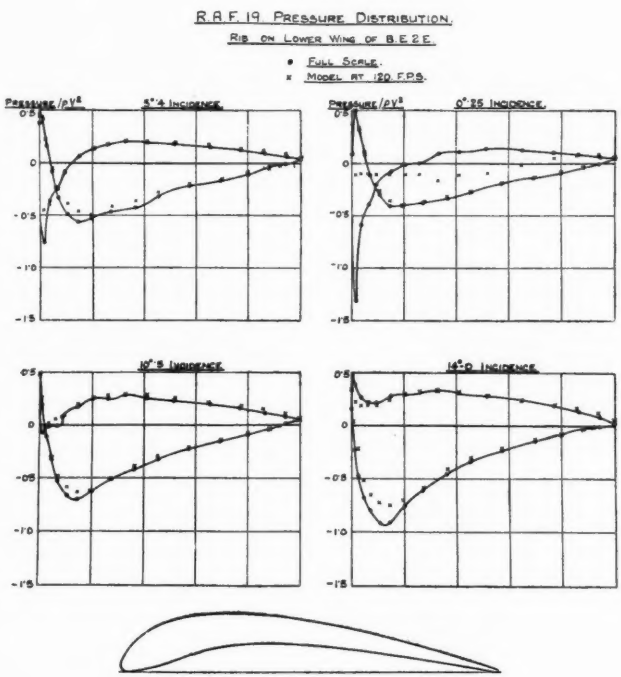



Fig. 13.

(3) The drag coefficient of a similar aeroplane, when gliding with the airscrew stopped, does not differ from that of the model by more than $\pm 0.002$. This is about $\pm 7$ per cent. of the minimum drag of the whole aeroplane and airscrew, or about $\pm 10$ per cent. of the whole aeroplane without the airscrew.

(4) Such differences appear to be fairly uniform for any one aeroplane over the whole range of lift coefficient, and hence are likely to be due to some part whose drag coefficient does not vary much with lift coefficient—probably the body.

(5) When two normal aeroplanes differ solely in *wing arrangement* (*e.g.*, in aspect ratio) a model test is likely to predict relative resistances accurately; I may add, following R. & M. 859, Part IV. (where the Bristol Fighter experiments are described in detail), more accurately than the Prandtl theory is likely to predict them.

(6) When the differences are in the *body*, model tests may be misleading. I admit this is surprising, and in my opinion unfortunate, but I see no escape from the conclusion. I think it seriously limits the usefulness of a private wind tunnel in ordinary aeroplane design.

(7) I believe that model tests give the centre of pressure of wings of the R.A.F.15 type accurately, and the results should be used in stress calculations. But for calculations of tail settings, an important part of design, the full scale results are likely to be a better guide. They may be regarded as giving a rational way of estimating the tail setting of a new setting from measured tail settings of previous ones.

(8) I am not prepared to rely on model experiments on " high lift " wings, nor am I ready to give a definition of a " high lift " wing! If called upon to decide whether a given wing should be regarded as " normal " or " high lift," I should be guided principally by its shape, which I should compare with R.A.F.19, R.A.F.14, R.A.F.15, and " old B.E.2c " —the last because I consider that the scale effect shown in Fig. 4 of R. & M. 374 should not be neglected.

## 8 *Future Research*

You will be interested in the future of the work I have referred to. The remaining discrepancies will certainly receive attention, but I think you will wish to know whether similar information is being obtained for aeroplanes fitted with other wing sections—in particular, " high lift " wings similar to those which have been tested in Germany. I may say that wind tunnel experiments on some of these have already been made, and that full scale experiments will certainly be put in hand very soon. Work is already going on on the full scale on a Junker aeroplane fitted with a wing section of this type, with which you will be familiar, and the corresponding model experiments will be made in due course. Finally, experiments are already authorised in connection with Mr. Handley Page's slotted wing, and I think we may hope that information on all these will be available as the result of flying during the coming summer.

## Discussion

The Chairman said that the Society was to be congratulated upon having the data contained in the paper brought before it so early, and he thanked Mr. Farren for the able manner in which he had gathered it together. Mr. Farren might not have been responsible for the actual experimental work, but there was no doubt that the manner in which he had gathered the data together, into a form in which it could be put into a report, had been most praiseworthy. There was no doubt that the importance to designers of being able to make accurate deductions from model results was very great indeed. The expense involved when full-scale machines went wrong was enormous, and no doubt there were cases in everybody's mind where such expenditure to which manufacturers and the Government had been put was due to incorrect deductions from models. One case was that of the very large Tarrant machine which was built towards the end of the war, where the money lost was measured in many tens of thousands of pounds, and where the issue was largely a question of model and full scale. Mr. Farren, in his paper, had referred to the general importance of the work of the N.P.L. and the R.A.E. not being recognised, and with Mr. Farren's remarks in that connection he (the Chairman) would like to associate himself. He had said before, and would say again, that he considered that the fundamental work of the N.P.L. and the R.A.E. before the war was the basis of the progress we made during the war, and, had it not been for that work, we should not have been in a position, at the end of the war, of having more or less established aero-

plane design on sound lines. In conclusion, the Chairman said it would be interesting to know what would happen if R.A.F. 19 was tested full scale as a monoplane. He took it that no such experiment had been made.

Mr. FARREN said it had not.

The CHAIRMAN considered it possible that it might show results very different from the full-scale results that were obtained when tested as a biplane.

Major A. R. Low expressed his pleasure at the admirable way in which the lecturer had stated the relations which should exist between such institutions as the N.P.L., the R.A.E. and the Test Establishments on the one hand, and the designers on the other, in order to give the latter the available data.

As Mr. Farren had had experience both inside and outside these bodies, he had naturally a very keen appreciation both of what the designers wanted and of what they might reasonably hope to get. It was not, however, quite fair to say that designers were illogical in offering to accept relative values as between model and full scale, without insisting on absolute values. It would be more precise to say that the apparent concession had proved to be, in fact, illusory, since there was the same uncertainty about both kinds of measurement.

With regard to the statement that there was complete agreement between pressure distribution in model and full scale, the physical basis of the success of Prandtl's elementary theory for wings at favourable incidence was that in this case the normal pressure distribution was almost entirely due to the inertia forces which were very slightly affected by the viscosity of the air once the appropriate circulation was set up. But the tangential forces, arising directly from the viscosity, were of the same order as the horizontal component of the normal pressures—that is the induced drag—and thus, especially at low values of Reynolds number and at small lift and induced drag, affected seriously the value of the drag and of the moment of the wing as calculated on Joukowsky's principle, neglecting viscosity.

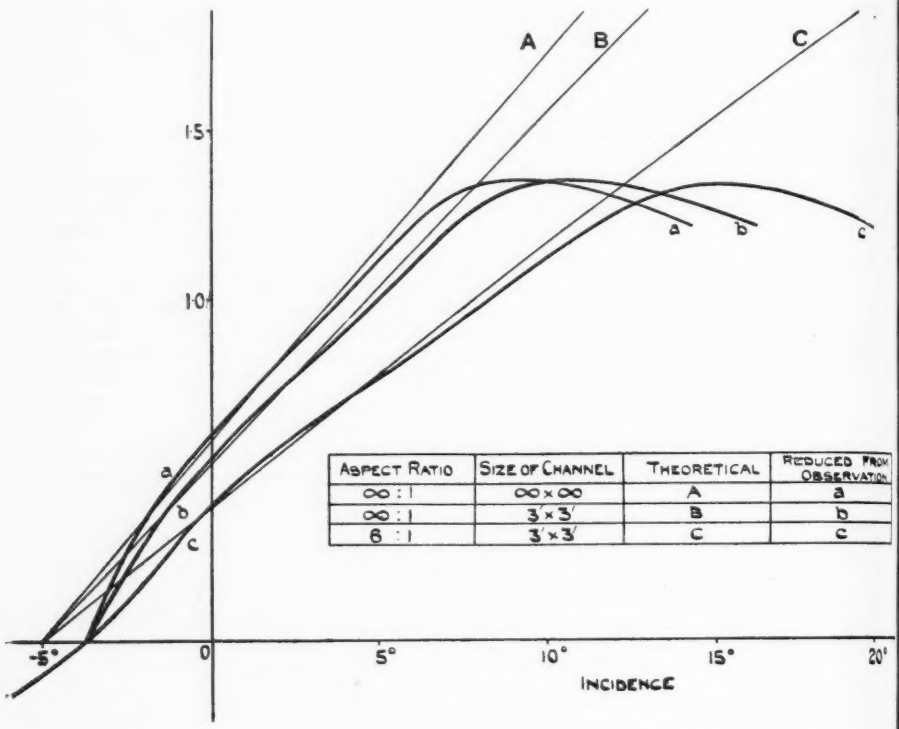
No doubt the body effects were still more important.

With reference to the different opinions of different authorities, Professor Bairstow some years ago, in a very instructive discussion of the applications of Rayleigh's criterion of dynamical similarity, had maintained that the method enabled us to reconcile measurements at different speeds on different scales, and even in different media. On the other hand, Professor G. Thomson had stated in his text-book that the discrepancies were so irreconcilable that model tests were unsafe for use by designers. Both opinions were too extreme, and no doubt the authors had modified their opinions in accordance with extended knowledge.

The lecturer had clearly suggested that Prandtl's methods were common knowledge, and that his reductions for aspect ratio and for tunnel interference were now in routine use. This was welcome as the first explicit statement of what might have been guessed for some time past, that our aerodynamicians were busily making up for lost time and were compiling experimental verifications of Prandtl's theorems, no doubt on the high standard to which they were accustomed from the N.P.L.

But he was entitled to ask since when Prandtl's achievements had been officially recognised, accepted and adopted, and what apologies were to be offered to Lanchester for neglecting his physical theory of wing lift until it came back from Germany.

This diagram, which he would table, showed the actual test figures for a R.A.F. 6 of eighteen-inch span obtained in a three-foot square channel at the N.P.L. compared with the results obtained on Joukowsky's assumptions, ignoring viscosity entirely. The curves corresponded in pairs A a, B b, C c, as reduced by corrections for aspect ratio and channel interference.




| ASPECT RATIO | SIZE OF CHANNEL | THEORETICAL | REDUCED FROM OBSERVATION |
|---|---|---|---|
| ∞ : 1 | ∞ × ∞ | A | a |
| ∞ : 1 | 3′ × 3′ | B | b |
| 6 : 1 | 3′ × 3′ | C | c |

It would be seen at a glance that these corrections, which were given by exceedingly simple approximate formulæ, removed by far the greatest part of the discrepancy and brought the curves into practical coincidence throughout the flying range. Yet in the past the channel interference had been lumped under "scale effect."

It would be rash to generalise too confidently from this one agreement between theory and test, but he put on record his view that for normal pressures on wings within the flying range, it was a waste of time to seek for scale corrections until they had corrected for channel interference by Prandtl's methods, which also dealt successfully with aspect ratio effect, or self-interference, and biplane and multiplane effect or mutual interference.

Tangential forces were harder to deal with and were not, so far, amenable to such easy and simple methods as the forces discussed above. The same was true of wings above stalling incidence, of irregular bodies, and of the mutual interference of such. Thus, for the body and accessories we were forced to accept the lecturer's conclusion that we did not yet know all the conditions, and were not very sure when we passed from model to full scale whether the results would justify the assumptions.

[*Added after the lecture.*] He fully recognised the difficulties of the work undertaken by the Committee of which the lecturer was Chairman, and offered his appreciation of its exposition in the lecture.

He understood from Mr. McKinnon Wood, who had been among the earliest here in his appreciation of Prandtl's methods, that Mr. Glauert had obtained

reductions similar in character to those shown in the diagram, and illustrating, moreover, the remark above on the increasing effect of tangential forces at low values of Reynolds number.

With regard to the great mass of work being done in Germany on what was there called the " new " hydrodynamics, he felt that its extent and importance were not realised here. If they compared the slight references to the subject in Glazebrook's Dictionary of Physics published in 1923 with the references in an article by Prandtl, Fluessigkeitsbewegung, in the Handwoerterbuch der Naturwissenschaften, published in 1913, it would give ample reasons for concern. On the face of it we had been more than ten years behind in this work of setting up a satisfactory physical theory for the problems laid before them in the paper.

Major F. M. GREEN, speaking from the point of view of a designer, did not agree entirely with Mr. Farren's definition of what the designer wanted. What the designer wanted primarily were facts, because he could not feel justified in designing anything new without facts. We had applied to aeroplane design all the knowledge that we had gained in other branches of engineering, but there were certain things we must know if we were to make progress in the art of aeroplane design, and perhaps the most important were the characteristics of wings. We had had plenty of wind tunnel results in the past, and had been told that: " These are the results, and you ought to make use of them," but the occasion of the reading of Mr. Farren's paper was perhaps the first time we had had some indication of what we might or might not use. Unfortunately, agreement was far from complete, and possibly it never would be complete, but we had at last a collective series of full-scale results, and we ought to be very grateful. The problem with which the designer was faced was to make an aeroplane better than the one he made previously, and, naturally, he looked out for all the latest information he could get. The designer often noticed in the papers—and in the technical Press especially—that new wing sections had been designed which had given positively astonishing results—a lift coefficient never previously heard of and speeds never previously expected. Unfortunately this was merely newspaper talk and not fact, and until the designer felt that he had evidence—test results and figures—that he could believe, he was not entitled to spend the money of the people who paid his salary in making an experimental aeroplane on such meagre information. If we had taken the advice of the Press in the early days, we should all have been making high-speed monoplanes and not low-speed biplanes. We must remember that the world's speed record is still held by the " low-speed " biplane. Again, if we took the advice of many of our advisers we should undoubtedly be making cantilever monoplanes, but he had not yet been able to find any data published by an authority he could believe in which gave him figures showing that any thick wing cantilever monoplane had made a performance equal to that of the best thin wing biplane, taking into account, of course, the things that mattered, such as the percentage of useful load carried, the speed, petrol used, and so forth. We were very short indeed of knowledge as to what were the characteristics of the thick wing—so-called high lift section; so short of knowledge were we in this matter that we awaited eagerly the authoritative results promised of full-scale tests. Even when we had made an aeroplane it was difficult to get at the facts. Early in 1914, at the R.A.E., they produced a design of aeroplane known as the S.E.4, the object being to make the fastest aeroplane they could, but which would still have a reasonable landing speed. In order to get the best results they had made use of some model tests made at the N.P.L. They interpreted them in the best way they could, and, as a result, it had occurred to them that the use of flaps the whole length of the wing, which could be moved up or down, would increase the top speed, improve the climb, and decrease the landing speed. Unfortunately, war was declared just about the time the aeroplane was being tested, and the few experiments made were not very

conclusive. Later, they made another aeroplane with a smaller horse-power engine, namely, the S.E.4A, in a similar fashion. This was flown, was used in the war, engaged in fights, and so on. Very few were made. They were tested, but those concerned had never been able to find out what was the low speed of that aeroplane. The pilots had made some very remarkable low-speed flights, but the R.A.E. were not so certain of the results; they found that the same pilots could make very nearly the same low-speed flights on machines with flaps in the normal position. Even to this day the slow speed of that particular aeroplane is not known. They had come to the conclusion that it did not pay to put flaps on an aeroplane designed for war, because the increased weight and cost was not justified by the increased performance. If they were able to get really accurate results, they would know whether that was or was not a wise decision. The position was much the same with the thick wing machines. A number had been made, but we were still short of the essential facts, namely, the performance of the aeroplane and the stalling speed. Unfortunately, also, we were still short of the measurements made by competent people of the Handley Page slot device. There had been papers on the subject, and undoubtedly there was a substantial decrease in landing speed to be obtained, but we were still without the facts. We did not know what the decrease of speed was, or the performance, and he was extremely glad that the Research Committee had prevailed upon the Air Ministry to spend the money and find out the facts. He expressed the hope that we should get the results reasonably quickly, and that they would not be held up indefinitely while being discussed. When the experimental work was finished the results should be published without remarks, in the first place, and we should not have to wait while they were being digested by those who authorised the tests. After all, the tests were made on behalf of this country, and the country included the aeroplane industry, and it seemed a great pity that we did not get these results more quickly. It was a step in the right direction to be able to get the results contained in the present paper so quickly. An aeroplane designer, after all, could only improve his design in the light of knowledge, and he believed that, for some reason or another, the results obtained from a number of aeroplanes which were made and tested at the country's expense, in a very thorough manner, were not available in the ordinary way to designers. He believed that was very largely the fault of the industry itself. It was a great pity that the results of tests on aeroplanes should not be published to the world at large, for by that means the British industry, and the aeroplane industry of the world, would get information which would enable designers to design better aeroplanes. Surely that would be to everyone's advantage. In conclusion, Major Green thanked Mr. Farren for his paper, which was an important contribution to the subject. He hoped this would be taken as a precedent, and that the members of the Research Committee, or the Committee itself, would be permitted to publish their results in good time.

Mr. FARREN, replying to the discussion, expressed thanks for the kind remarks made by the speakers. With regard to the Chairman's remark about experiments on R.A.F. 19 as a monoplane, it was purely a question of how many experiments one could do at once. He had been taught, by one of those who had spoken in the discussion, that if an establishment was working at top pressure, and if someone in authority came along and said he wanted a certain thing done urgently, the question to put to him was : " What work do you want me to stop? " What did the Chairman suggest should be given up in order to carry out work on an R.A.F. 19 monoplane?

The CHAIRMAN said he merely wanted to know if the experiments had been done.

Mr. FARREN said they had not. He did not believe they would throw so much light on the subject as experiments of the same kind with other wings.

Replying to Major Low's remarks as to what was said by Bairstow and Thomson in their books, Mr. Farren said it seemed to him that the best answer was to be found in the books themselves. He did not accept Major Low's version of what either of those gentlemen had said. Broadly, what he had intended to imply in his own remark was that Bairstow said he considered that the results of the pressure exploration on R.A.F. 14 did not establish any difference between full scale and model, and the best interpretation of them was that there was no difference. Thomson had said that the difference was large. He (Mr. Farren) was not aware of any remark in Thomson's book to the effect that model experiments were useless, and it was highly improbable that it contained such a remark, considering the number of model tests referred to in it.

Dealing with the Prandtl theory, Mr. Farren said he regarded it as an accessory in this inquiry. It came in to a small extent, but in his opinion it did not make the main feature of the agreement. He regarded the improvement it did make as strengthening the basis of that branch of the Prandtl theory. As to whether the Prandtl theory had been adopted by the Aeronautical Research Committee, as he did not speak for the Committee he could not give any official information. He thought he might suggest that the Aeronautical Research Committee did not " adopt " theories. He thought that the right course was to apply the theory and see if it agreed with experiments. If it appeared to do so, it was for the convenience of those for whom the experiments were made to apply any corrections it indicated. As to why Lanchester's work was not followed up earlier, he could only say that Lanchester always seemed to think of things before other people did.

He agreed with Major Green as to the need for the publication of Martlesham Heath results to all designers. To refuse to agree to the publication of information which showed what we should *not* do was to take a short-sighted view. Mr. Farren expressed the hope that in so far as the writer depended on designers, they would agree to interchange information, both of failures and successes.

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This concluded the discussion, and a hearty vote of thanks was accorded Mr. Farren for his valuable paper.

## FURTHER REMARKS BY THE LECTURER.

On reading through the discussion, I find there are one or two points to which I omitted to reply.

I find it difficult to agree with Major Low's suggestion that the observed difference in centre of pressure is due to a scale effect on tangential forces on the wings or other parts. To account for the difference in moment coefficient shown in Fig. 9 the drag force on the whole aeroplane would have to be moved a distance of the order of half the wing chord. Presumably the part of the drag which might properly be described as " tangential " would have to be moved several times this distance.

I think Major Low hardly does justice to the work of the N.P.L. and R.A.E. on the Prandtl theory. Mr. Glauert's first report on this (a summary of the then state of the theory) is dated February, 1921, and his many original contributions since then are surely known to Major Low.

I am not aware of any reason for believing that the Prandtl theory deals less effectively with forces which are largely " tangential " than with those which are due to normal pressure. I think it is agreed that the essential basis of the theory is the agreement between its predictions and the results of experiments—experiments in which there are always tangential forces acting. It might be *expected* that the theory would produce less satisfactory results when a substantial

part of the force was " tangential," but in fact that is not found to be true. The agreement on drag of wings of various aspect ratios, biplane and monoplane, etc., is very good.

I cannot accept Major Low's final proposition—that the Prandtl theory has " practically eliminated the so-called scale effect." It has materially reduced the discrepancy between the characteristics of a model in a wind tunnel at the highest *IV* so far attained and those of the full-scale aeroplane in free air. But it throws no light, for example, on the known scale effect over the model range of *IV* on most bodies, nor on that observed on R.A.F. 19.

I feel that Major Green would not really wish " undigested " experimental results to be published. There is really remarkably little time lost between the first presentation of a report to the A.R.C. and its publication. From my own experience I should put it at twelve weeks, of which six are occupied in printing, etc. Anyone to whom the Air Ministry considers the report is specially useful is, I believe, supplied with a Roneo-ed copy; if this is not done now, it certainly was done at one time.